

# Antiquity

## A Quarterly Review of Archaeology

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### Editorial Notes

ARCHAEOLOGY is still something of a Cinderella at the British universities. Certain branches of this vast subject are indeed taught (for the most part incompletely) at a few of them: notably, classical archaeology in Oxford, Cambridge and London; Egyptology, more particularly at the first and last; the prehistory of Europe and the Near East at Cambridge and Edinburgh; British archaeology in London and at Cardiff; Romano-British archaeology at Durham and Newcastle. But all this is both very scattered and very restricted. When we remember that problems of first-class importance are now being attacked by British archaeologists in Greece, Palestine, Egypt, Irak, India, China, South America, South Africa, Kenya, and even in Great Britain itself, we realize that the facilities for preliminary training and subsequent research offered by our British universities are inadequate to the point of the ridiculous. And to the sum-total of professional archaeology must of course be added that incidental contact with archaeological materials or problems which from time to time complicates the routine of Dominion and Colonial administration. Some regularized instruction in the archaeology of our Colonies might (if the necessary machinery were available) reasonably be added to the burdens of our young Colonial civil servants. But the machinery is, in fact, *not* available; or, rather, no determined attempt has yet been made to set up the machinery from the parts that

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exist. There is nothing of that reasoned cooperation without which economy of effort and speedy and effective development cannot be expected. The time for cooperation and concentration is overdue.



One attempt was, it is true, made on a small scale before the War. At Liverpool an Institute of Archaeology, with three or four Chairs, was established as a focus for teaching and research, and has produced some notable work. But both the place and, perhaps, the time were ill-chosen. Liverpool has many claims to distinction, but it cannot claim to possess any special qualifications as a centre for the study of the world's archaeology. Even Oxford and Cambridge have not, and can scarcely hope to have, all the materials necessary for an Archaeological Institute on a really comprehensive scale. London stands—potentially—alone in this respect. Only in London can we expect permanently that convergence of materials and workers which alone can make such an Institute a real, world-wide power in this department of science. And, translated into academic terms, this is as much as to say that the responsibility for taking the greatest step ever yet attempted in the administration of archaeology, on an international basis, devolves now upon the University of London.



The University of London is a century old, but is only now in a position to put its house in order. For it is indeed only now, at long last, on the point of owning a house to put in order. Hitherto the University has been a lodger on remote and alien ground. Its new buildings, when they arise on the northern side of the British Museum, will be its first real home, and will at once present it with facilities for development that few, if any, other universities in the world can rival. How far it will take advantage of these facilities remains to be seen. In the meantime, those University departments of knowledge which rejoice in far-seeing directors and deep pockets are staking their claims for permanent accommodation. The Institute of Historical Research, established a few years ago on the initiative of Professor A.F. Pollard and already a flourishing centre of postgraduate work, will vacate its hutments and clothe itself in bricks and mortar. The Institute of Art, recently founded through the munificence of Mr Samuel Courtauld, will begin its labours under Professor W. G. Constable.



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It remains to add the third member of the trinity, and to establish, in close juxtaposition to these two Institutes, that Institute of Archaeology which would inevitably share so closely in many of their needs and interests.



In this special context, the whole matter is worthy alike of earnest thought and urgent action. Here is an unrivalled chance for co-ordination not only within the limits of archaeology itself but also between archaeology and two kindred departments of knowledge. Take the case of the libraries of these various establishments. The overlaps between archaeology, history, and the history of art are multitudinous : what economy of space and expenditure to have them side-by-side ! A small scientific laboratory for research work and instruction in preservative measures—a necessary element in the training of every field- or museum-archaeologist—is equally necessary to archaeology and to art. And so on. It is unnecessary to elaborate the essential affinity of interest between the three Institutes. With this and other factors in mind, the University Board of Studies in Archaeology has prepared a carefully considered scheme for an Institute of Archaeology which shall occupy a part of the top floor of the new University buildings, alongside the Institute of Art. The scheme provides for the close storage of teaching and research-collections of a kind nowhere at present available, and includes the mechanism necessary for postgraduate work in all branches of archaeology.



To the details of this scheme we shall return in a future issue. It is a scheme which is both practical and urgent. But the most urgent part about it is that *now or never* must it find a sufficient financial guarantee to secure from the University of London the required minimum accommodation. This accommodation—which is, be it emphasized, the essential foundation of the whole scheme—will cost £28,000. The sum is large, but in no way disproportionate to the magnitude of the development which it represents, and it is in fact considerably less than is being spent on the Institute of Art. Under other conditions, isolated from the kindred institutes and the British Museum, such an Institute of Archaeology would cost incomparably more and would be incomparably less effective. But the plans for the

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new University buildings are already under preparation, and time is short. Unless a substantial part of that £28,000 is guaranteed almost immediately, we shall have missed perhaps the greatest opportunity that organized archaeological research has ever had in Europe. Who will help ?



The Editorial notes on our domestic affairs, printed in the December number, have met with an encouraging response, and we have received many friendly letters from subscribers, some of whom were induced by our 'appeal' to change their intentions. Others have told us they cannot do without ANTIQUITY—it was described by one as an 'educational luxury'—and will continue it in spite of the difficulties in which we all share. Some, of course, are compelled to withdraw their support—only temporarily we hope—but at the same time express their great regret at having to do so. We also wish to thank those who have sent their subscriptions for 1932; the next paragraph is inserted for the attention of those who have not yet done so.

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The SUBSCRIPTION to ANTIQUITY for 1932 is now DUE. We would remind our Subscribers of the form and envelope inserted in the December number for the purpose of remitting payments. *An early response will be much appreciated as this will save avoidable trouble in having to send out direct reminders.*

Payment should be made to

*The Assistant Editor, 24 Parkend Road, Gloucester.*



# The Etruscans and Pompeii

by R. C. CARRINGTON

THE evidence for Pompeian history has been furnished almost entirely by the use of the spade, and, while for the latest period its full and detailed character makes it difficult to pick out a unifying thread through it all, the further back in time one goes the scantier direct testimony becomes ; until, in dealing with the earliest periods, we are, as it were, building up a case out of evidence which is largely ' circumstantial '. This defect is inherent in the archaeological study of any site, since it is to be expected that what was built latest should be best preserved, but the defect has been aggravated at Pompeii by the way in which, until recently, the excavations have been carried out.

The confessed object of these excavations has been not to trace the development of the town from the beginning to the end of its history, but to preserve and present to the visitor's eyes a picture of Pompeian life as it appeared at one definite period—the few years immediately preceding the eruption. Thus, as soon as the latest level was reached, the digging stopped, and no attempt was made to penetrate to the lower and earlier strata. The results, though interesting for the tourist, were unsatisfactory for the historian. In the last few years, however, deep excavation has been carried out at various places on the line of the city-walls and within the area of the town, and this, happily, is said to be but the prelude to further digging of a similar type. Deeper excavation will solve many problems which at present we can only attempt to solve by conjecture or inference. One of the most important of these problems is that of the part played by the Etruscans in the growth of the town, interest in which has been recently renewed by the publication of the results of the excavations made on Pompeii's city-walls.<sup>1</sup> Hitherto, discussion of this question has been a little unreal, since, with the exception of a passage in Strabo which will be

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<sup>1</sup> Amedeo Maiuri, *Studi e Ricerche sulla Fortificazione di Pompei*, in *Monumenti Antichi*, 1930, vol. XXXIII, part 2.



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mentioned later, the data requisite for discussion, *viz.*, those furnished by archaeological evidence, were lacking.

Before the recent excavations were begun, it was recognized that the system of fortification, as it existed at the time of the eruption of A.D. 79, contained work of at least three different epochs. The latest stage in its history, consisting in the abandonment of its use for military purposes and in the partial building-over of the line which it had occupied by private houses, was recognized to have been a development of the early decades of our era. The next stage (working backwards in the chronological series), which consisted in the insertion of square rubble-work towers at intervals in the wall and the reconstruction of large stretches of the wall itself in the same material, was correctly dated to the beginning of the first century B.C., to the period, that is, of intense preparation which preceded the final struggle between Rome and her Italic allies over the question of the franchise. Beyond this nothing certain was known, and the earlier stages of development were wrapped in darkness. The new excavations, however, have shed an entirely fresh light on the origin and history of these walls, and have furnished us with our first tangible evidence for testing the truth of the various views which have been held by ancient and modern writers on the growth of the town.

Signor Maiuri, the Director of the Naples Museum, who was in charge of the excavations, has traced the following five periods in the development of Pompeii's fortifications :—

(1) 520–450 B.C. (the period of their original construction). The fortification, which may have been 18 feet high on its outer face, was composed of a double curtain of limestone ashlar masonry (fig. 1, A–A') containing an inner core of beaten earth (a). The courses of the ashlar masonry were very irregular in width (plate 1).

(2) 400–300 B.C. The outer curtain of the wall was demolished, and in its place was erected a new curtain about 30 feet high, of limestone ashlar masonry, differing from the one it replaced in the greater thickness of its blocks and in the regularity of the width of its courses (fig. 1, B). Inside this was piled a sloping *agger* (b), with a low revetment on its inner face (B'). The *agger* completely covered the inner revetment of the original wall.

(3) 300–180 B.C. The outer curtain (B) had its height increased (c), and in places was entirely replaced, by a construction in ashlar masonry of volcanic tufa. At the same time, and in consequence of



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the increase in the height of the outer facing, the *agger* was widened (c-c'). Moreover, the platform on the top of the fortification, on which the defenders stood, was strengthened by the construction in the core of the *agger* of an inner wall of ashlar masonry, mostly of tufa (c'').

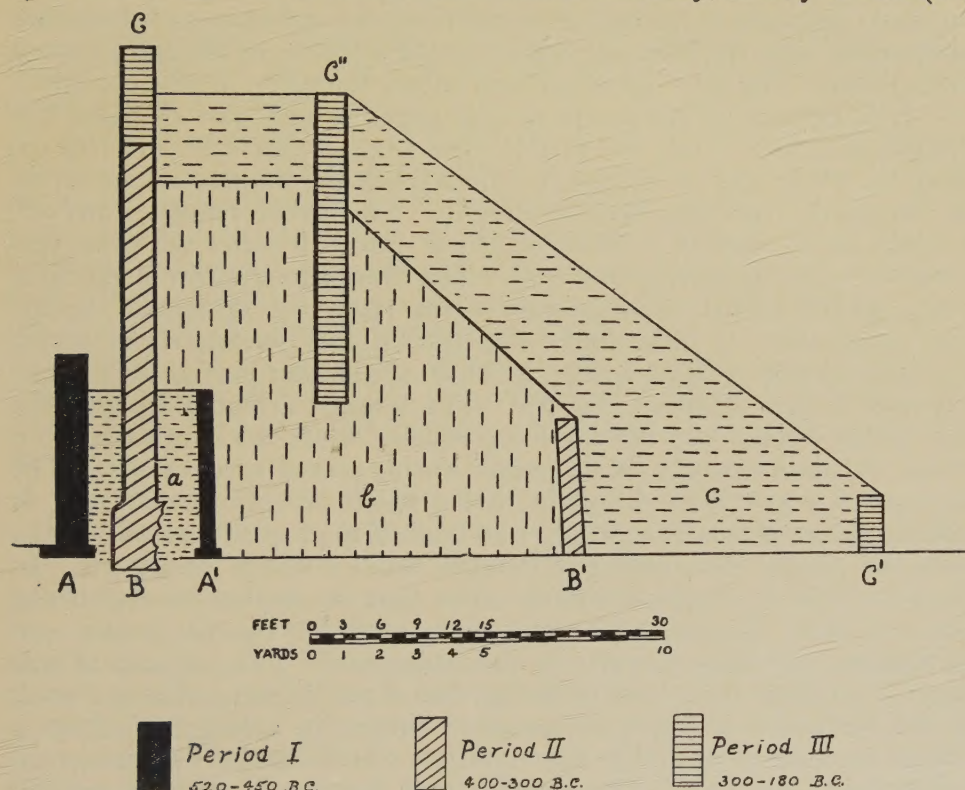


FIG. 1. Cross-section of the Pompeian fortifications of periods I, II, and III, reconstructed according to data furnished by the report of the excavation

(4) 120-89 B.C. The ashlar masonry of the outer facing was largely replaced by rubble-work of lava. Square towers were inserted at intervals, occupying the space between the outer curtain (c) and the inner wall (c'').

(5) 80 B.C.-A.D. 79. The military use of the fortifications, as has already been stated, was gradually given up.

The walls are believed by the excavator to have been constructed originally (period I) by the Oscans, working under the direct influence

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of the Greek colonies of Cumae, Neapolis, and Posidonia (Paestum), and the reason he suggests for the action of the Oscans is a desire for safety against the attacks of the Etruscans from the interior of Campania. Periods II, III, and IV were due to the Samnites, who, as is well-known, invaded Campania at the end of the fifth century and remained dominant in Pompeii until the Dictator Sulla planted a colony of Roman veterans in the town. Period V is subsequent to the founding of this colony.

It is no part of the scope of this paper to consider all these five periods in detail. Only the first of them (A, a, A') directly concerns us, since the other four all belong to a time when the Etruscan domination in Campania had long been shattered. Two other points, however, have to be mentioned in order to throw into full relief the great importance of the discovery of a wall which dates precisely from the time when the Etruscans were present in the region of Pompeii. In the first place, there is little doubt that the original nucleus from which Pompeii developed was a village, to which the greater part of the present city was a later addition (fig. 2).<sup>2</sup> The original village comprised the area of the Forum and the adjoining *insulae*, while the spur of land on which the sixth-century Doric temple stands served as acropolis. The later town was bounded by the walls which we are discussing. It follows, therefore, that the people who erected the fortification of period I were the people who enlarged Pompeii from a village to a town. In the second place, Strabo definitely states that the earliest inhabitants of Pompeii were Oscans, who were succeeded by the Etruscans and Pelasgians, and these in turn by the Samnites.<sup>3</sup> The purpose of this paper is to show that there is strong reason for thinking that the work of the Etruscans in Pompeii consisted simply in enlarging it from a village to a town, and that the tradition which Strabo has preserved is based on solid fact. The proof of our thesis will depend entirely on the coherence of circumstantial evidence, since it must be admitted at once that among the rare objects found during the excavation nothing was discovered which can be connected with the Etruscans directly and beyond possibility of doubt. It will be convenient to approach the problem from three different angles, considering (1) various scraps of evidence (other than that of the town-wall) which suggest that the enlargement of Pompeii was the work of the Etruscans, (2) the precise nature of the town-wall of period I, especially in comparison with the

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<sup>2</sup> F. Haverfield, *Ancient Town-Planning*, pp. 63-9; v. Gerkan, *Griechische Städteanlagen*, pp. 119-120.

<sup>3</sup> v, p. 247.



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walls of Posidonia, (3) certain coin-series prevalent in South Italy during the sixth, fifth, and fourth centuries B.C., which enable us to supply a motive for the action of the Etruscans in planting a strong settlement on the site of Pompeii at the end of the sixth century.

Archaeological discoveries, such as the inscribed tile from Santa Maria di Capua,<sup>4</sup> the archaic terracottas found in the same place and elsewhere in Campania, which bear a close resemblance to terracottas of admittedly Etruscan origin,<sup>5</sup> and the early Campanian inscriptions which seem to be written in a mixture of Etruscan and Oscan,<sup>6</sup> confirm the view handed down to us by several ancient writers,<sup>7</sup> but denied by some modern scholars, that Campania came within the sphere of Etruscan conquest: and research into the art and language of South Italy gives results which are difficult to explain if the dominion of the Etruscans did not at some time extend even further south than Campania.<sup>8</sup> If an Etruscan conquest of Campania be accepted, it follows *a fortiori* that Pompeii came within the sphere of their domination. Apart from this, the detailed evidence relative to the question is as follows:—

(1) The plan of Pompeii is purely Italic. The town comprises, as has already been mentioned, the area of an original village and a later accretion. Each of these distinct portions of the town is laid out round two main axes—a *cardo* and a *decumanus* (fig. 2). The *cardo* of the original village is preserved in the present Strada del Foro and Strada delle Scuole; its *decumanus* in the Strada della Marina and the western portion of the Strada dell'Abbondanza. In the enlarged town, the Strada Stabiana represents the *cardo* and the Strada Nolana the *decumanus*. The occurrence of two main axes, both in the original village and in the later town, shows that the Greek method of laying out a town with more than one *decumanus* and *cardo* of equal importance had no influence here and that the origin of the Pompeian plan must be sought on Italic soil.

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<sup>4</sup> Trombetti, *La Lingua Etrusca*, p. 141 ff.

<sup>5</sup> Koch, *Dachterrakotten aus Campanien*.

<sup>6</sup> Pauli, *Altitalische Studien*, vol. III.

<sup>7</sup> Polybius, II, 17, 1; Velleius Paterculus, I, 7; Strabo v, p. 242; Dionysius of Halicarnassus, VII, 3.

<sup>8</sup> V. Macchiore, *Gli Elementi Etrusco-italici nell'Arte e nella Civiltà dell'Italia Meridionale*, in *Neapolis*, I, 1913, pp. 270–306.

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(2) The shapes of the *insulae* which resulted from this town-planning are in the main not rectangular, like those of a Greek city, but trapezoidal (fig. 2). 'Neither its oblongs, nor its squares, nor its street-crossings exhibit true right angles, though many of the rooms and peristyles in the private houses are regular enough. In this feature Pompeii resembles the trapezoidal outlines of the Terremare. It resembles also much Roman military work, both of Republican and of



FIG. 2. Street-plan of Pompeii, showing the area of the original village, and (in red) the main axes of this village and of the enlarged town

Imperial date, which disregards the strict right-angle, and accepts squares and oblongs which are, so to say, askew . . . Whatever the reason, the trapezoidal house-blocks of Pompeii exhibit a feature which is not alien to the earlier town-planning of Italy, though it is strange to the cities of Greece'.<sup>9</sup>

(3) Ancient writers record various peoples as inhabiting the southern and eastern portions of Campania, and in some way or other they can all be connected with the Etruscans. Polybius says that these regions

<sup>9</sup> F. Haverfield, *op. cit.*, pp. 67-8.



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were occupied by the Daunians and Nolans.<sup>10</sup> Of these two peoples, the Daunians are mentioned by Dionysius of Halicarnassus as having taken part with the Etruscans in an expedition against Cumae in 524 B.C.,<sup>11</sup> while the territory of the Nolans is said by Polybius to have been conquered by the Etruscans at the same time as the valley of the Po (*i.e.*, the end of the sixth century B.C.).<sup>12</sup> The elder Pliny mentions the Umbri as amongst the peoples that ruled over Campania,<sup>13</sup> and they,

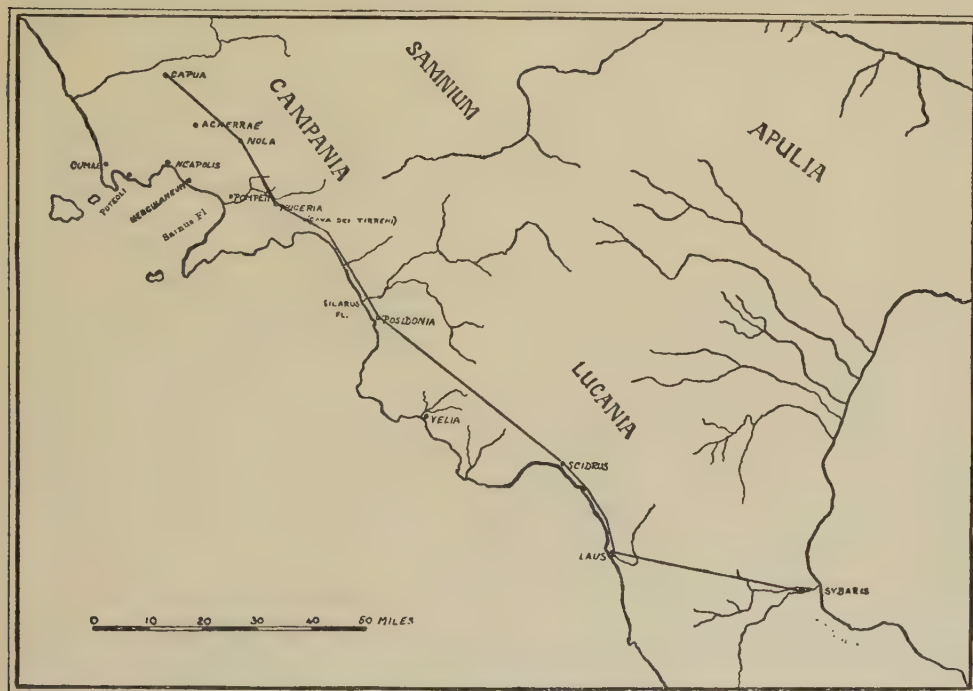


FIG. 3. Sketch-map to illustrate the trade connexions between the Etruscans and the Sybaris

too, took part in the expedition of 524 B.C. Furthermore, Nuceria occurs as the name of a town both in South Campania (fig. 3) and in the centre of Umbria; and, though its etymology is uncertain, the Campanian town is designated by a fourth century historian a Tyrrhenian (Etruscan) city.<sup>14</sup> Not only, then, Campania as a whole, but more specifically the territory in the immediate vicinity of Pompeii

<sup>10</sup> III, 91, 4.    <sup>11</sup> VII, 3.    <sup>12</sup> II, 17, 1.    <sup>13</sup> *Nat. Hist.*, III, 60.

<sup>14</sup> Philistus, apud Steph. Byz. s.v. *Νουκρία*.

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was regarded in antiquity as having at one time been occupied either by the Etruscans or by peoples who were in alliance with them. What of the Pelasgians, whom Strabo couples with the Etruscans as inhabitants of Pompeii? Elsewhere the same author preserves the tradition that the town of Caere, originally founded by Pelasgians who came from Thessaly, was subsequently captured from them by the Etruscans.<sup>15</sup> Furthermore, in his description of Ravenna he connects the Umbrians with the Thessalians, quoting the tradition that the original founders of the city were Thessalians, who, unable to bear the wanton outrages of the Etruscans, voluntarily admitted some of the Umbrians, who continued in his time to hold the town, and themselves went back to Thessaly.<sup>16</sup> In these two passages the Pelasgians and Umbrians are both parts of the pre-existing population conquered by the Etruscans, and both have a connexion, direct or indirect, with the Thessalians. It may therefore be suggested that the Pelasgians, whom Strabo mentions at Pompeii, are the same as the 'Umbrians and Daunians and many of the other barbarians' who are stated by him to have aided the Etruscans against Cumae in 524 B.C., and whom we have seen reason to connect with the southern part of the Campanian plain. 'Pelasgians' would, then, appear to be a general designation of all these peoples. It seems certain that, if the enlargement of Pompeii was in truth the work of the Etruscans, the actual settlers in the enlarged city would be, not pure Etruscans, but allies who had taken on the Etruscan civilization. Probably this is what is implied by Strabo's coupling the Etruscans and Pelasgians together.

At this point, however, a doubt arises. The people who enlarged the area of the town must have been the builders of the original wall,<sup>17</sup> which, from pottery found in the earth filling, Maiuri dates to the period 520-450 B.C. Yet Maiuri is firmly convinced that the constructors of this fortification were Oscans, working under the direct influence of the Greeks. Here, however, it is necessary to distinguish between fact and theory—the date of the building of the walls and the inferences which the excavator draws from the style of their construction. The excavations supply evidence that the first wall of the enlarged city was built roughly at the end of the sixth century B.C. On the other hand, if Maiuri's view be accepted that these walls were the work of the Oscans, any theory of an Etruscan foundation of Pompeii must

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<sup>15</sup> v, p. 220.

<sup>16</sup> v, p. 214.

<sup>17</sup> Fig. I, A, a, A'.



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be abandoned. We must, then, examine in detail the nature of this early fortification and especially those features of it on which Maiuri bases his opinion that it was built under direct Greek influence.

The wall, as we have seen, was composed of an earth core with a double facing of limestone ashlar masonry. Only the foundations of the outer face (A) survive, the rest of it having been destroyed to make way for the Samnite wall of period II. Large stretches, however, have been found of the inner face (A') which, to the east of the Porta del Vesuvio survives to a height of 12 feet (plate 1). The courses of the masonry forming this inner facing are of varying width, and thus the work falls under Vitruvius' description of an *opus pseudisodolum*.<sup>18</sup> The wider courses are composed of blocks which average  $2\frac{1}{2}$  feet in depth and 10 inches in thickness, and which vary in length from just under  $1\frac{1}{2}$  feet to just over 3 feet. At intervals along these courses, blocks are inserted endwise, so that their narrow edges only are visible in the facing, while with their length they run into the earth core, thus helping to bind core and facing together. The narrow courses in the facing serve this same purpose, being composed of blocks laid flat, which dove-tail into the earth core and bind it to the stone revetment. It is the existence in this early fortification of a double curtain, containing the earth filling, which, in Maiuri's view, is a sign of Greek construction, and which distinguishes it from the Italic system of a single revetment with a sloping *agger* inside it (as, for example, in the fortification of the Samnite period B, b, B').

This double facing of ashlar masonry and the absence of an *agger* can only be explained, he asserts, by the direct influence of Greek defensive methods such as could have been exercised by the walls of Cumae, Neapolis, or Posidonia. The whole subject is beset with difficulties, and the surviving specimens of city-walls of this period in South Italy are too few to warrant sweeping assertions. If we limit ourselves to the walls of Posidonia, which are the only ones readily accessible, we are at once confronted by the fact that they contain work of two different epochs. Maiuri has promised a detailed report of an excavation which he has recently carried out with the object of tracing the history of these walls, and such a report will be invaluable. For the moment, it is enough to notice the peculiarities of the two types of construction which the walls contain. First of all, there was erected

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<sup>18</sup> II, VIII, 17. *Isodolum*: cum omnia coria aequa crassitudine fuerint structa. *Pseudisodolum*: cum impares et inaequales ordines coriorum diriguntur.

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a fortification composed of a double facing of ashlar masonry, the courses of which are of even width (*opus isodomum*), and contain an earth filling (plate II). Then in front of the outer curtain, a new facing was built of much rougher ashlar masonry with courses of irregular width (*opus pseudisodomum*) (plates III, IV), and on the inner side an *agger* was constructed, completely covering the earlier inner revetment and thus preserving it intact. Until the report of the excavation is published, it will be impossible to date these walls with precision, but the general nature of the two stages in the fortification is clear. The original wall with the double curtain of regular ashlar masonry is Greek; the irregular masonry and the *agger* are Italic. The former, therefore, must date from a period earlier than the Lucanian invasion at the end of the fourth century B.C., and the latter from a later period. It follows that if either of these walls directly influenced the earliest of the walls at Pompeii, it was the Greek wall and not the Lucanian additions. If, however, we compare the Pompeian wall with the contemporary wall of Posidonia, we find that the differences are really more striking than the similarities.

In the first place, as we have mentioned, and as a glance at plates I and II will make clear, the blocks of which the two curtains of the Posidonian wall are composed, are long and rectangular in shape and are laid in narrow courses of even width (*opus isodomum*), while those of the Pompeian wall, though rectangular in shape, are much more square and are laid in courses of irregular width (*opus pseudisodomum*). The depths of the wide and narrow courses at Pompeii are  $2\frac{1}{2}$  feet and 10 inches respectively. Those in the Greek wall at Posidonia average about 1 foot 2 inches. The former wall, therefore, is at best but a poor imitation of the latter.

Furthermore, the blocks of the Pompeian inner revetment are only half the thickness of those at Posidonia (10 inches as compared with 1 foot 10 inches); and, most significant of all, the face which they form is not vertical but, as it rises, slopes slightly towards the outer curtain, so that the fortification is narrower at the top than at the bottom. This is very important, for it means that, while in the Posidonian fortification the two strong curtains are real walls and would stand even if the earth filling were removed, at Pompeii the earth filling is of fundamental importance, since without it the inner face would not stand. For the same reason, the narrow courses occurring at Pompeii, composed of blocks running horizontally into the earth core and binding it and the facing together, and also the wedges, which



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were inserted at intervals in the wide courses to serve the same purpose, were not required at Posidonia, since the walls were thick enough to stand by themselves. In other words, at Posidonia we are dealing with two real walls and an earth core ; at Pompeii we have an earth *agger* with stone revetments. The *agger*, as Maiuri himself admits, was typical of Italic defensive schemes.

The Posidonian wall contains at intervals square towers, such as were typical of Greek fortifications, though not of Italic : in the earliest Pompeian wall no towers have been found.

Far, then, from showing signs of direct Greek influence, Pompeii's early wall is un-Greek in its use of pseudisodomic masonry, in the slope and weakness of its inner revetment, and in the absence of towers ; it is built after the manner of an Italic *agger*, merely strengthened with a facing of stone. To find a closer parallel to it we must look at the Lucanian additions at Posidonia, built in the pseudisodomic style (plates III, IV). Occurring in conjunction with the *agger*, the latter afford a proof outside Pompeii that this style of masonry was in use among Italic peoples at a period not earlier than the end of the fifth century B.C. Whence did they derive this style ? It is to be noted that the Posidonian *opus pseudisodomum* is of much poorer technique than the Pompeian—a fact which needs some special explanation, since the latter is about a century older than the former and so might naturally be expected to show a rougher and more primitive style of construction. It may be suggested that the finer technique found at Pompeii was due simply to the influence of the Etruscans, and that the survival of the use of *opus pseudisodomum* among Italic tribes a century later was a legacy from the same source.

This is in complete accord with another result of Maiuri's excavations. The present Porta di Ercolano, Porta del Vesuvio, and Porta di Stabia at Pompeii (fig. 2) are but later constructions replacing earlier gates in the original walls, and in addition a tower at the north end of the Strada di Mercurio (*i.e.*, the northern continuation of the *cardo* of the original village) covers the site of a former gate which was later disused. Hence we may conclude that at any rate in the western half of the town, the earliest street-plan coincided with the present one. Under the Porta di Nola no vestige of the early walls has been found, a fact which seems to indicate that to the east the original line of the walls was later changed. How far the first walls went in this direction can only be ascertained by further excavation between the Porta del Vesuvio and the Porta di Nola on the north side, and the Porta di Stabia and the

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Porta di Nola to the south and east. It can be shown, however, that a large portion of the original street-plan was never altered after it was first laid out, and that plan, as we have seen, was not Greek but Italic. Clearly the walls and the town-plan go together. How, then, did it come about that, if the Oscans followed the Greek model in the building of the walls, they show no sign of Greek influence in the planning of the town? Pompeii's neighbour, Herculaneum, with its parallel streets of equal importance, forming perfectly rectangular *insulae*, is as Greek in plan as it could possibly be, and clearly owed its inspiration to the Greek town of Neapolis.<sup>19</sup> Had the Oscans of Pompeii been under Greek influence, they would not, any more than the Herculaneans, have remained unaffected by the splendid example of Greek town-planning which lay across the Bay.

The idea, then, of direct Greek influence in the building of these early walls must be abandoned. They seem, rather, to be Italic walls trying, as it were, to 'look' Greek. There remain two possibilities: either we must believe that the Oscans themselves erected the walls or we must accept the statement of Strabo and infer that they were erected by the Etruscans. A few moments' consideration, however, will show that the Oscans must be ruled out. During the sixth century, a temple was built on the 'acropolis' attached to the original village, and the orientation of this temple shows clearly that its erection antedated the enlargement from village to town (fig. 2). This temple was purely Doric in plan and ornament, and must have been put up either by Greek settlers or (more probably) by Greek workmen employed for a special piece of work. The important point is that, when the Oscans of Pompeii carried out a work under Greek influence, they came under that influence completely. There were no half measures: the temple was purely Greek. If, then, the Oscans were completely under Greek influence during the course of the sixth century, why had they ceased to be in that position by the end of the century? The only likely answer is 'by reason of the arrival of the Etruscans'. An invasion by a conquering people would give a reasonable explanation of that sudden increase in population which is indicated by the enlargement of the village into a town—an enlargement which is not explained by Maiuri's theory of an Oscan authorship for the earliest walls. May it not be that the Etruscans, or 'Etruscizing' Italians, having failed before the walls of Cumae in 524 B.C., and being impressed by their first contact with a type of

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<sup>19</sup> Beloch, *Campanien im Alterthum*, ed. 2, p. 230.



PLATE I



THE INNER REVETMENT OF THE EARLY FORTIFICATION AT POMPEII

*facing p. 16*

PLATE II



A RESTORED PORTION OF THE GREEK WALL (6TH CENT. B.C.) AT POSIDONIA (PAESTUM)



PLATE III



A PORTION OF THE LUCANIAN FORTIFICATION (5TH OR 4TH CENT.) AT POSIDONIA,  
BUILT AS A SCREEN COVERING THE ORIGINAL GREEK WALL

PLATE IV



ANOTHER LENGTH OF THE LUCANIAN REFACING OF THE GREEK FORTIFICATIONS AT POSIDONIA



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fortification to which they were not accustomed, tried to combine in Pompeii the merits of the Italic *agger* with those of a Greek wall, and thus produced the hybrid construction which the earliest wall seems to show? If so, this early fortification affords just the required confirmation of Strabo's assertion that the Etruscans and Pelasgians were the successors of the Oscans in the domination of the town.

A *prima facie* case has already been made out for the view that the Etruscans not only, as Strabo says, were at one time in occupation of Pompeii, but were also responsible for the enlargement of the area of the town. We will now approach the question from an entirely new angle. Assuming, for the moment, what we set out to demonstrate, that the Etruscans did in reality fulfil the rôle which we have ascribed to them, we will seek to discover a possible reason for their action and to determine when it is most likely to have taken place. We shall see that the theory of an Etruscan foundation of Pompeii enables us to explain many points connected with the coinage of South Italy, which would otherwise remain unexplained, and also that the action which we are assigning to the Etruscans fits perfectly into the scheme of knowledge which we possess of the economic situation in South Italy at the end of the sixth century B.C. It will, moreover, become clear that the colonization of Pompeii was an integral part of the wider economic policy of the Etruscans in the Bay of Naples at this time, and that, at the time of its extension, the enlarged town was designed to have an importance which it never enjoyed after the Etruscan domination came to an end. Such a study, though it begins by assuming what we are required to prove, will be found to have provided one more piece of circumstantial evidence in support of the view which this paper is upholding.

We have already referred to the passage of Dionysius of Halicarnassus in which he states that, in 524 B.C., Aristodemus, tyrant of Cumae, was successful in repelling a joint attack of the Etruscans and other Italic peoples. This shows that the Etruscans were already present in Campania. Consistently with this, Polybius states that the Etruscan conquest of the district round Capua and Nola was contemporary with their advance into the valley of the Po, which we know from archaeological evidence to have taken place towards the end of the sixth century B.C. There is reason for ascribing the beginning of the Etruscan domination in Campania to an earlier date than this, but for our present purpose it is sufficient to note that by the last quarter

of the sixth century they seem to have been well established there. We have mentioned, too, that, though Campania was probably the southern limit of their political domination, their cultural influence spread much further south. These cultural relations were, moreover, accompanied by ties of a more material nature. A well-known passage of Athenaeus mentions the close friendship that existed between the Etruscans and the inhabitants of the Achaean colony of Sybaris; the passage implies that the basis of the friendship was commercial.<sup>20</sup> Sybaris, in fact, seems to have been the entrepôt between the Etruscans and the cities of Asia Minor, especially Miletus. These conditions imply the existence of a trade-route between the Tarentine Gulf and Central Italy, and it is important to trace the line which it followed. Its course can be deduced from a study of certain of the coin-series of South Italy (fig. 3).

Laus, situated across the toe of Italy from Sybaris, had been colonized from the latter city some time before 550 B.C., and its silver coins, most of which belong to the end of the sixth century B.C. or the first decade of the fifth, indicate close commercial connexions with the mother city. They have the incuse reverses and the cable borders of the coins of Sybaris, and the type—the figure of a bull—varies only in so far as the animal is represented with a human head.<sup>21</sup> Posidonia, colonized from Sybaris in the seventh century B.C., began to strike coins in the latter half of the sixth century, not, however, on the Euboic-Attic standard in vogue among the Achaean colonies of South Italy, but on the Phocaic standard of her neighbour Velia.<sup>22</sup> Nevertheless, relations with the mother-city are indicated by the use of a flat fabric and of incuse reverse types. At the beginning of the fifth century, a change took place, and, though a fabric different from that of Sybaris was introduced, the reverse type of the mother-city—the Bull—was adopted. This change has been interpreted as an indication of an influx of refugees from that city after its destruction in 510 B.C. It is not a far cry from Posidonia to Campania, and evidence of close commercial relations between these districts is furnished by the coinage of the Campanian cities Fenser and Hyria. Some of the types, issued by these cities in the course of the fourth century B.C., show on the obverse the head of Hera Argoia, a clear reference to the well-known temple of this goddess at the mouth of the river Silarus.<sup>23</sup> The type

<sup>20</sup> XII, 519 b.

<sup>22</sup> *ibid.*, p. 80.

<sup>21</sup> Head, *Historia Numorum*, 2nd ed., p. 73.

<sup>23</sup> *ibid.*, pp. 37-8.



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figures also on the coins of Posidonia. Fenser and Hyria can be shown to have been situated at any rate in the vicinity of Nola, and there is some reason for regarding the latter two cities as identical. These cities, then, appear to mark the line of the Etruscan communications with Sybaris, the whole route from Nola, one of the main Etruscan centres in Campania, to the Tarentine Gulf being secured by a chain of important posts—Nola, Posidonia, Laus, and Sybaris.

The southern march of the Etruscans into Campania brought them into conflict with the Greek cities on the coast, and the hostile relations, which are attested by the battle at Cumae in 524 B.C., continued till 474, when the doom of the Etruscan domination in this region was sealed by the victory of Hiero of Syracuse. Numismatic evidence suggests that this political conflict was accompanied by a gradually widening cleavage between the economic interests of the two powers.

Cumae, which at this time enjoyed a kind of hegemony in the region of the Bay of Naples, began about 490 B.C. to issue coins of the same standard as the early issues of the other Chalcidian colonies of Magna Graecia, equivalent to the Aeginetan drachm and also to one third of the Euboic tetradrachm.<sup>24</sup> Coins of this issue, however, are rare, since two new standards were adopted in quick succession. Soon after 490, a standard was followed which was certainly equivalent to the Euboic-Attic, and then, after a short period in which this and the Phocaic standard were used simultaneously, the Phocaic triumphed (about 480) and silver currency on this standard lasted until Cumae was captured by the Samnites in the last quarter of the fifth century B.C.

The coinage of Etruria presents us with two series, whose marks of value show that the unit of silver in the one case was 13.5 grains and in the other 17.56 grains.<sup>25</sup> The unit of the first series is clearly the Sicilian silver *litra*, the equivalent of a pound of bronze. The most common values (in *litrae*) of the Syracusan coins are 50, 20, 10, 5, 3,  $2\frac{1}{2}$ ,  $1\frac{1}{2}$ ,  $1\frac{1}{4}$ , and 1, and with these the values issued by the Etruscans are in close accord, *viz.*, 50, 25, 20,  $12\frac{1}{2}$ , 10, 7, 5,  $2\frac{1}{2}$  and 1.<sup>26</sup> It is impossible, however, to correlate the latter with the form of the Euboic-Attic standard in use among the Achaean colonies of the Tarentine Gulf, for they divided the stater or didrachm into thirds, sixths, and

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<sup>24</sup> *ibid.*, p. 36.

<sup>25</sup> *ibid.*, *Introd.* LIV, and pp. 14-15.

<sup>26</sup> *ibid.*, pp. 12-13 and pp. 176 ff.

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twelfths. In view of the Etruscans' known commercial relations with South Italy, these facts might seem to invalidate any attempt to use coinage as evidence of such relations. It must, however, be remembered that this series where the unit is 13.5 grains seems to derive in large part from Populonia, the port of Etruria on the west, and that no finds have yet been made which would suggest that it was also in use among the Etruscans in Latium and Campania, where they were more immediately in contact with the south. As a people, the Etruscans showed a distinct lack of capacity for national cohesion, and their tendency towards internal divisions is illustrated by the coexistence of two systems of coinage. It is quite possible that the Etruscans who conquered middle Italy merely used the coinage of the cities of the south which acted as intermediaries between them and the East. For our present purpose, the important consideration is that, when about 480 B.C. Cumae abandoned the Euboic standard in favour of the Phocaic, it was a definite sign that her economic interests were developing on lines quite different from those of the Etruscans.

Nor, again, is it possible to correlate the second series of Etruscan coins (with a unit of 17.56 grains of silver) with the contemporary coinage of Cumae, though the question which standard this series was designed to follow is still a matter of discussion. Even if Head be right in regarding it as modelled on a weakened Aeginetic standard (since five units of 17.56 grains give a total of 87.80 grains, which is roughly equivalent to the Aeginetic drachm of 90 grains), the date of its first appearance (about 450 B.C.) was later than the abandonment of the Aeginetic standard at Cumae (490) and so precludes any possibility of a connexion between the two. The economic interests of Cumae, then, in the early fifth century had drifted apart from those of the Etruscans. The adoption of the Phocaic standard is, of course, to be explained by the revival of Phocaic influence in the western Mediterranean after the battle of Alalia and the settlement of the survivors at Velia in Lucania (538 B.C.), and it must be taken as an indication that Cumae was abandoning the economic relations which she had had with the natives of the interior down to the Etruscan conquest and was seeking closer relations with such Phocaean colonies as Massilia.

With the consolidation of the Etruscans in the interior of Campania at the end of the sixth century, their foreign policy in this region was bound to develop on two main lines. It was natural, in the first place, that they should attempt to extend their zone of influence on the sea further towards the south, and, in the second place, that they should



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seek to strengthen their interests in the region of the Tarentine Gulf. One result of the first line of policy was the unsuccessful attack on Cumae in 524 B.C. In connexion with the second line of policy, a crisis arose in 510, for by the destruction of Sybaris at the hands of the Crotoniates, the Etruscans lost their chief connecting link with the East. After this disaster and in view of the friendliness of Posidonia and Laus, where the expelled Sybarites seem to have found a refuge, it was clearly to the advantage of the Etruscans to strengthen their communications with these cities in the hope of thereby being able to regain the entrepôt which they had lost. Herein lies the importance of the colonization of Pompeii. From Nola the direct route to Posidonia and the south led through the gap in the Monte Sant'Angelo in which is situated the modern Cava dei Tirreni. At the point where this pass is entered from the Campanian plain, there lay, in Roman times, the town of Nuceria (the modern Nocera), and nearby flows the river Sarno. The mouth of this river was clearly a point of strategic importance for a power which wished to hold the Nuceria gap, but whose hold on the western sea was precarious. If this point were not held, a hostile force could land without difficulty and strike a swift blow inland. The enlarged Pompeii, then, was designed to be a strongly fortified outpost, covering the line of the Etruscan communications with the cities beyond the Nuceria gap. The building of the Greek temple had shown that the enemies of the Etruscans were not blind to the possibilities of the site, but the Etruscans themselves (on the theory here presented) took no action until, with a hostile Cumae mistress of the Bay of Naples, and Sybaris destroyed, they were forced to take steps to restore their position in this quarter.

But the importance of Pompeii did not end with the protection it afforded to Etruscan communications with the south: it was integrally bound up with the first line of Etruscan policy which we have distinguished, *viz.*, the extension of their zone of influence on the sea. Strabo speaks of Pompeii as the port of Nola, Nuceria, and Acherrae,<sup>27</sup> and, considering the distance of the country behind Vesuvius from Puteoli, the leading port of the Bay of Naples in Strabo's time, it is comprehensible that a subsidiary trading-post was required in the south-east. It is indeed strange that Acherrae, a town situated eight miles to the northeast of Naples, should have used a sea-port so far to the south. But, however this is to be explained, Strabo's remark is

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<sup>27</sup> v, p. 247.

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significant as showing how wide were Pompeii's inland connexions in the Augustan age, when Campania and the Bay of Naples were no longer divided between conflicting powers, and there was no political reason for using one sea-port in preference to another. At the beginning of the fifth century B.C., Naples would undoubtedly have been the most convenient outlet for the Etruscan centres at Capua and Nola. This way, however, was already closed, for the site of Naples had been occupied on two distinct occasions by settlers from Cumae, and its coinage, which begins about the middle of the fifth century, shows that by that date it had not shaken off the domination of the mother-city.<sup>28</sup> A strong power in the interior of Campania could not allow the means of egress both to the north and to the south of Vesuvius to be closed. Too late to occupy the exit to the north, the Etruscans seized that to the south, and Pompeii thus served the double purpose of a sea-port for the cities of Campania and a guard of their communications with the south.

In order that Pompeii might serve the purpose which we seek to assign to the action of the Etruscans, it was imperative that it should be kept in subordination to the towns of the interior. There is no direct evidence of such dependence during the Etruscan period, but indirect evidence is furnished by the entire absence of any sign that Pompeii ever had a coinage of its own, a strange phenomenon in view of its size and of the number of towns which are known to have issued coins in the early Samnite period. Nuceria, from the beginning of the third century B.C. till the Social War of the Sullan age, seems to have been the head of a league of cities in South Campania, of which Pompeii was one, and it is quite possible that this dependence on the interior was a legacy from Etruscan days.

We have, then, considered evidence on four main points : (1) the plan of Pompeii and the shapes of its house-blocks are Italic ; (2) there was a strong tradition in antiquity that the Etruscans and their allies were at one time in possession of the southern part of the Campanian plain : moreover, Strabo affirms that the Etruscans and Pelasgians (*i.e.*, peoples conquered by the Etruscans or allied with them) succeeded the Oscans in the occupation of Pompeii ; (3) the first city-wall of Pompeii, constructed precisely at the time when the Etruscans were present in Campania is not Greek but Italic : it shows, however, a

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<sup>28</sup> Beloch, *op. cit.*, p. 29.



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higher technical skill than the Italic tribes of the immediate neighbourhood possessed, and therefore seems to have been constructed under the influence of some highly civilized power in the interior of Italy ; (4) the economic and military position of the Etruscans in South Italy, in the years which immediately followed the destruction of Sybaris in 510 B.C., was such as to give them strong reasons for desiring to establish a well-fortified colony at the mouth of the river Sarno where Pompeii is situated. The results of our investigations on these four points are absolutely coherent. They point to the conclusion that the Etruscans colonized Pompeii about 500 B.C. to guard their communications with South Italy and to obtain a *point d'appui* against their rival Cumae. If this conclusion is correct, the action of the Etruscans represents a not unimportant incident in the general conflict which was being waged, at the end of the sixth century B.C., and the beginning of the fifth, between the Etruscans and Carthaginians on the one hand, and the Cumaeans, Phocaeans, and Syracusans on the other—a conflict which, in its turn, formed part of a wider struggle going on throughout the whole Mediterranean at this time, between the swiftly developing civilization of Hellas and the older civilizations hemming in the Mediterranean (Oriental, Carthaginian, and Etruscan).

## Cross-Dykes

by J. P. WILLIAMS-FREEMAN

**S**HORT cross-dykes are found on ridges or more rarely in valley bottoms. Their essential characteristics are that they run across a narrow strip of open ground, their ends resting on obstacles which in primitive times would have been naturally impassable :— in the case of cross-ridge dykes from scarp to scarp or scarp to forest, and of cross-valley dykes across the hard gravelly bottoms between impenetrable woods. In every case that I have seen they cross the line of a primitive road.

They may be 'univallate', *i.e.*, consist of a single bank and ditch, or 'bivallate', *i.e.*, having a single ditch sunk between two banks ; and they may be of any size : 'large', *i.e.*, according to the Earthworks Committee's classification of over 10 feet crest-ditch vertical height, 'medium' between 10 and 4 feet, or 'small' below the last figure.

They occur on most ranges of hills in England ; they are found on the Berkshire Ridge, and are very common on the East Yorkshire wolds, but it is on the Chalk ridges of Wilts, Dorset, Hants and Sussex that they seem most numerous and have attracted most attention. Many doubtless exist which have not been recorded, but in the three first-named counties alone three have been noted across the East Wilts 'Oxdrove' along the West Hants border, and Mr Heywood Sumner<sup>1</sup> has described in South Wilts and Dorset no less than eleven across Whitesheet Hill (the old Shaftesbury-Sarum road), and nine across the Oxdrove ridge of Cranborne Chase. But it is on the South Downs between the Itchen valley in Hampshire and Willingdon Hill in East Sussex that the most striking series occurs. Here there are at least 39 and probably more. Most of these are to be found west of the river Arun where the southern slope of the chalk range is wider, gentler and more wooded, and all the complex groups are found in this section. It is this West Sussex series that Dr Eliot Curwen and his son,

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<sup>1</sup> *Earthworks of Cranborne Chase*, p. 62 et seq.



## CROSS-DYKES

Dr E. Cecil Curwen, specially investigated and described in their well-known paper<sup>2</sup> which must form the basis of all serious study of these earthworks.

The 'large' class cross-dykes are very few—not much more than half a dozen (including four in one group) in the four counties: each seems to have been constructed for a special defensive purpose and must be studied separately. It is the 'medium' and 'small' class, mostly with a crest-ditch vertical height (CD, vert!) of under 6 feet, that seem to fall into a class by themselves and can be studied together.

Their essential characteristics are well marked: their ends rest on the steepest slopes often at the heads of coombes, or upon large or small patches of thick impenetrable wood, and they all cross an old track, often the main ridgeway where one or two spurs with their secondary ridgeways have converged upon it. The track may pass through a simple gap—in no case defended—which may or may not be the original way through, or it may pass the end of the cross-dyke; in some cases most uncomfortably near the steep edge of the scarp. There can be no doubt that the position of nearly all cross-dykes is eminently suitable for obstruction of the road.

Apart however from this general resemblance cross-dykes and their characteristics show considerable variety: they can be discussed under the following heads.

- (1) Single univallate entrenchments.
- (2) Single bivallate entrenchments.
- (3) Double, treble or multiple contiguous banks and ditches.
- (4) Groups of the above elements, either of similar or different types.

I. THE SINGLE UNIVALLATE ENTRENCHMENT. This 'defensive' type (though for these little earthworks 'obstructive' or 'protective' would be a better term) is of course the simplest form of ancient entrenchment—a bank thrown up from a ditch on the enemy side—such banks and ditches are an extremely common form of cross-dyke. They may sometimes be very small—only 1 or 2 feet 'CD, vert!'—that is, not bigger than a hedge bank, and so often escape notice; but it is to be observed that even the smallest often choose a defensive

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<sup>2</sup> 'Covered Ways on the Sussex Downs', *Sussex Arch. Collections* 1918, LIX, 35-75.

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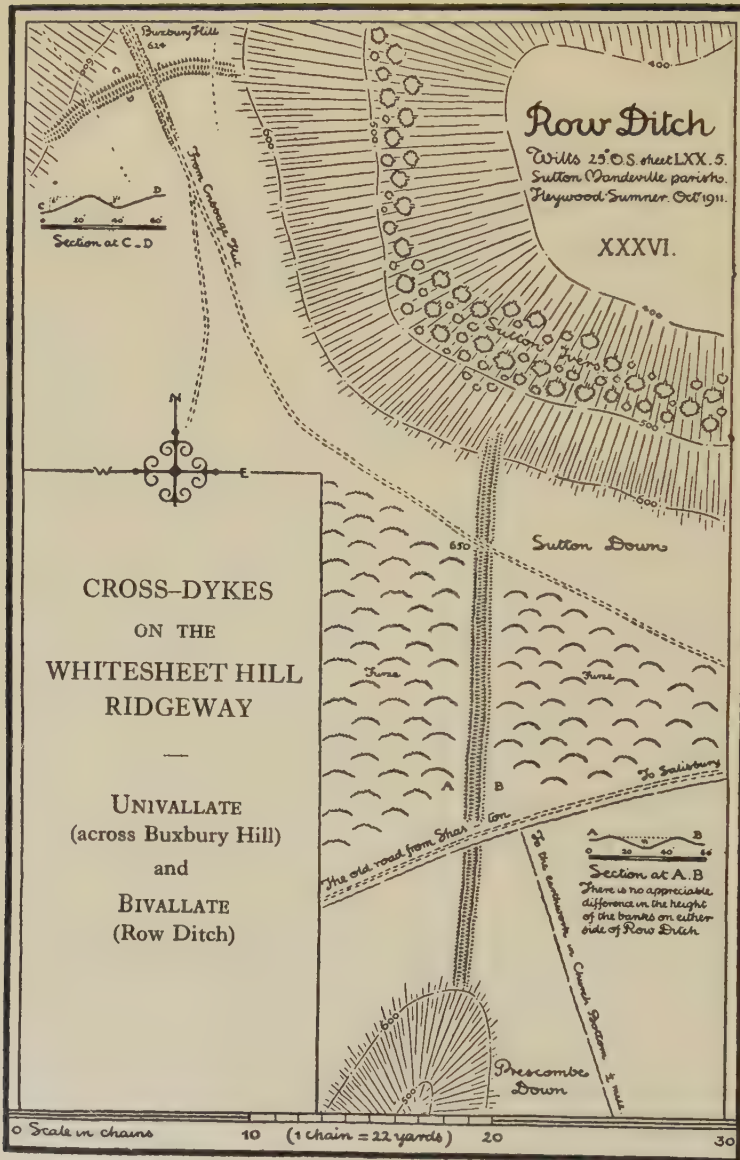


FIG. 1. By permission from Heywood Sumner's *Earthworks of Cranborne Chase*



## CROSS-DYKES

position. They may well have been, like hedge-banks, merely protective to feeding grounds or cultivation, while the larger ones may have been designed to hold up travellers and cattle going along the ridge and demand toll for free passage.

The rule that for defence the ditch must always be on the enemy side does not I think always hold good when the ascent up to the entrenchment is very steep: there are instances where the earthwork from its position has every appearance of having been constructed for defence, but where the makers have been content to take, as it were, the steep slope of the hill for their ditch and construct the entrenchment in the easiest way by throwing up the bank from above. Small banks with ditches on the wrong side may however be modern work—sham fights innumerable have been fought on the South Downs and volunteers' shelter trenches have to be borne in mind.

In one of the best examples of a single cross-dyke of defensive type (West Harting Down) the entrenchment terminates in a sort of rough shallow pit on the edge of each scarp. These may possibly mark the position of some sort of block-house to protect the ends.<sup>3</sup> Caesar, describing a short entrenchment he made in Gaul, which appears to have been a cross-dyke, states that he put 'castella' at the extremities for his military engines.<sup>4</sup>

2. SINGLE BIVALLATE DITCHES. These are by far the commonest form of cross-ridge dykes upon the chalk and differ essentially from the univallates in that it is the ditch that is the important element; in worn or ploughed down specimens it is this member that can often be followed where the banks have practically disappeared. They consist essentially of a ditch sunk between two banks—banks which typically are of equal size. In many cases, however, especially along slopes, the banks are now unequal and sometimes, especially in the larger examples, one bank so predominates that it is difficult to distinguish the entrenchment from one of the defensive type with a bank on the counterscarp.

In size these cross-dykes have usually a 'CD, vert' of 4 or 5 feet with an overall horizontal measurement of 40 or 50; thus exactly

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<sup>3</sup> There is just such a shallow pit at the southern end of Grim's Bank on Little Heath, northwest of Silchester, 70 yards north of the Roman road leading to Speen.—EDITOR.

<sup>4</sup> Caesar, B.G. II, viii.

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corresponding in profile to the long cross-country 'inter-settlement' ditches which are so common on Salisbury Plain and the open chalk generally—entrenchments which Colt Hoare christened 'Covered Ways', and are often described as roads or boundary ditches, but which there is a growing tendency to look upon as ancient cattleways. Sections cut by the Curwens<sup>5</sup> agree very closely with those of Old Ditch on the Plain and of other such ditches. There is the same average depth of the ditch of about  $4\frac{1}{2}$  feet below the natural surface of the ground, the same flat narrow bottom of 1 to 2 feet, and the same trumpet-shaped section, steepening rapidly to the bottom, which we know from Dr Cecil Curwen's recent investigation to result from the weathering of a nearly vertical-sided ditch.<sup>6</sup> The resemblance in construction between these cross-country and cross-ridge ditches is so close as to amount to identity.

The ends of the bivallate cross-ridge ditches are often prolonged, slanting down the sides of the Down as sunken or beaten paths, and occasionally as definite earthworks; and they may coincide with property or parish boundaries. This again corresponds with the ditches on the Plain where a track often continues their line for long distances.

Another occasional and very curious feature of this type of cross-dyke is the 'kink'. The ditch will take a sudden nearly right-angled turn for 30 or 40 feet and then turn again to continue its original alignment, the foremost bank taking the line of the rearmost. In every case an old track goes through the entrenchment at the 'kink', usually, if not always, flanked by a low turn of the bank. There are three such 'kinks' in bivallate cross-dykes in Sussex and another (as also one in Dorset) where the line of the banks suggests it, but an unusually broad road through has destroyed them too much to be certain. In the East Yorkshire dykes several such 'kinks' are figured by Mortimer<sup>7</sup> as well as other entrances and corners of a more complicated character.

3. DOUBLE AND MULTIPLE BIVALLATES. There are three pure examples of these cross-dykes known to me: on Twyford Down, near Winchester (double) and at Leydean near East Meon (treble) on the South Downs, and at Thickthorn Down in Dorset, also treble. Two other similar triple entrenchments exist—on Launceston Down

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<sup>5</sup> *Covered Ways on the Sussex Downs*, pp. 63 et seq.

<sup>6</sup> *ANTIQUITY*, March 1930, p. 97.

<sup>7</sup> J. R. Mortimer, *Forty years Researches*, 1905.



## CROSS-DYKES

and at the great British village on Gussage Down in Dorset—but they are not cross-dykes. They lie on comparatively level Down—indeed in the three first examples, though they are definitely across ridges and ridgeways, the ridges are not very narrow or sharp.\* They are beautiful, regular little earthworks from 300 to 500 yards long; a band of even contiguous banks and ditches, with a general height of from 4 to 6 feet,

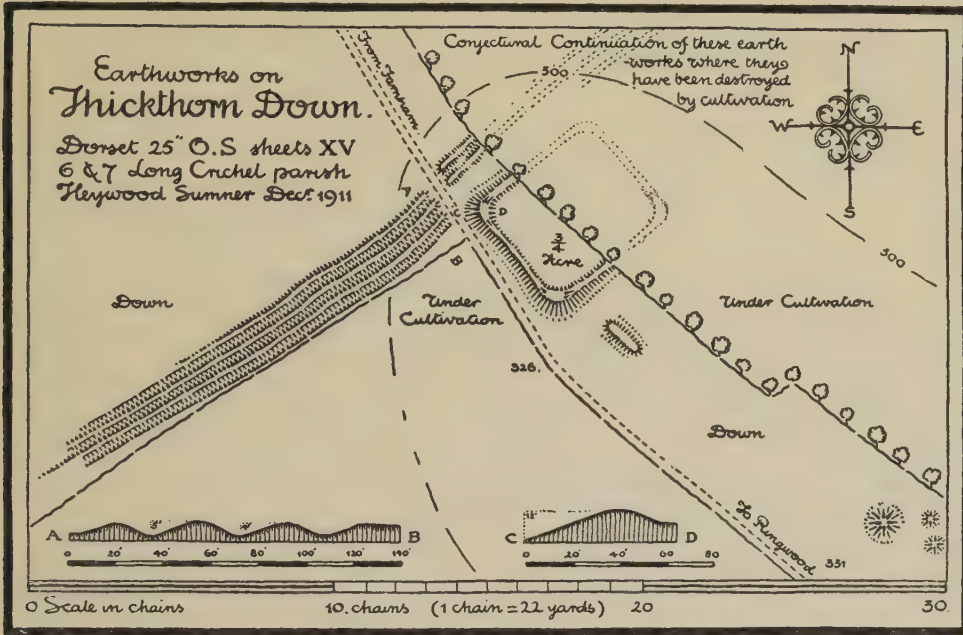


FIG. 2. MULTIPLE BIVALLATE CROSS-RIDGE DYKE  
By permission from Heywood Sumner's *Earthworks of Cranborne Chase*

and in the case of the triple ones an overall horizontal width of about 120 feet. Though the southern chalk cannot boast of a set of more than three ditches between four banks, Mortimer describes them as common up to six ditches and seven banks, and even figures one of no less than seventeen ditches and eighteen banks. Most of these are across ridges or high ground. Mortimer specially notes that his multiple ditches often diverge at their ends: both the South Down examples have tracks leading from their ends, and at Leydean at one end the ditches diverge—the other has been destroyed.

\*Two cross-valley dykes of this type have recently been noted in Hampshire.

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From Dr Cyril Fox's description of the Mile ditches near Royston<sup>8</sup> and mention of two others in Bedfordshire and Norfolk it would seem that this class of short entrenchment may be much more widely distributed.

Besides these pure examples of multiple bivallates, evidently constructed together as one system, in no less than five of the groups to be described next there occur contiguous banks and ditches of a much less regular and more haphazard composition, which though they may have served the same purpose by no means conform to the same type.

4. GROUPS OF CROSS-DYKES. (A) Uniform. There are in Dorset and Sussex three or four groups consisting only of two or three small univallate entrenchments a few yards apart, one behind the other. They are placed across a main ridge or more often across spurs as if for protection. (B) Mixed. The more important form of group however which Dr Curwen first described and planned is of a much more mixed and complicated character, so much so that it is sometimes difficult to analyze them into their component elements. There are five such groups on the South Downs, all as has been said on that part of the range where its long southern slope bears a scrub of varying thickness (now much replaced by beech plantations) with patches of thick woods especially in the valleys.

Each one of these groups is made up of both kinds of earthwork, each is placed in a strong defensive position as regards the ends of the cross-dykes, and in four out of the five the beginning or end of the group (and in one case both) is protected by an univallate entrenchment facing outwards along the ridge; the fifth is doubtful. The banks and ditches may be in sets, sometimes separated by 30 or 40 yards or more; the component members of the set may be of both kinds, they may be actually touching or separated by a few yards—the arrangement seems to be haphazard but the bivallates always predominate and the impression given is that the ditches were the important part of the system. The total depth of the group along the ridge may be as much as 370 yards, and the number of component cross-dykes may vary from three to seven or eight. There is again a marked tendency for the ends of the cross-dykes to diverge from one another and to be continued as tracks along the slopes of the ridge.

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<sup>8</sup> *Archaeology of the Cambridge Region*, p. 127.



# CROSS-DYKES

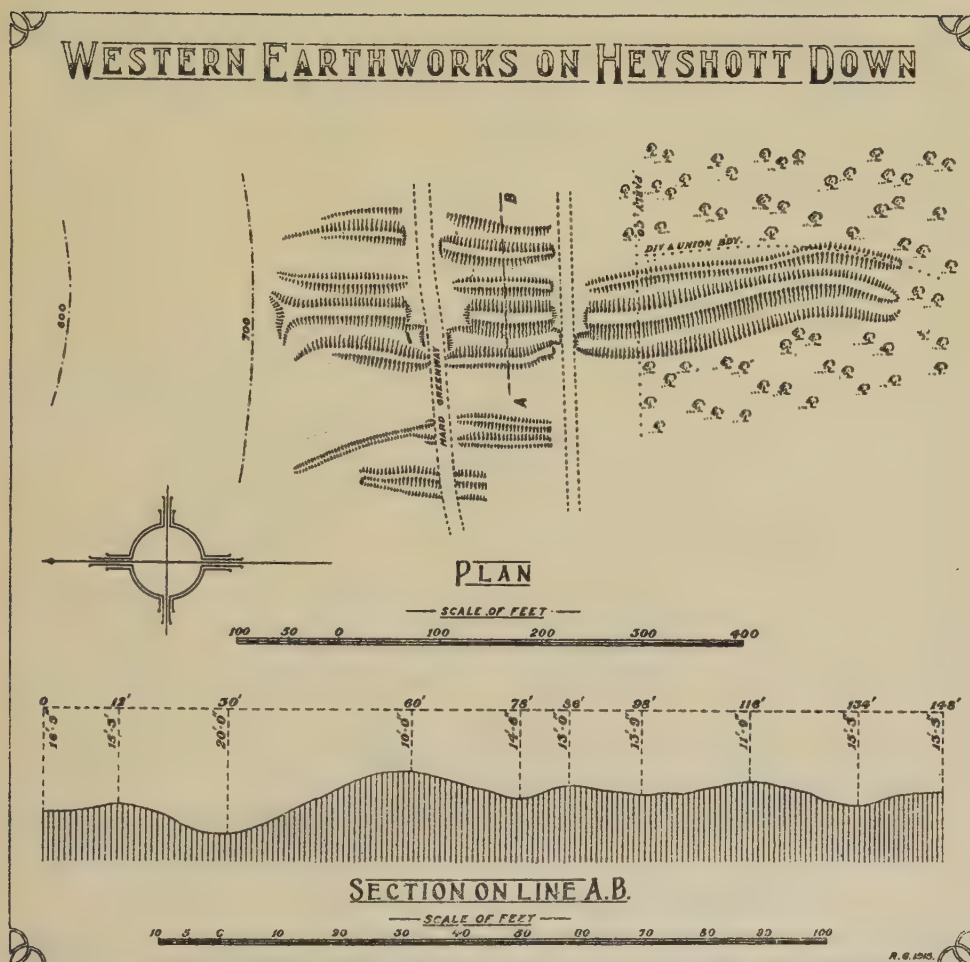


FIG. 3. GROUP OF MIXED CROSS-RIDGE DYKES  
 From E. and E. C. Curwen, 'Covered ways on the Sussex Downs'  
 By permission of the Sussex Archaeological Society

## ANTIQUITY

As regards the age of construction of cross-dykes the only clear evidence so far concerns the typical single bivallate example on Glatting Down. That is undoubtedly older than Stane Street, which passes over it, but as to how much older the evidence of a section cut by Dr Curwen<sup>9</sup> was somewhat inconclusive, though he puts it in the late Bronze Age. In one or two cases on the South Downs a barrow interferes with a cross-dyke and appears to be later, and in another (a multiple bivallate) it appears to be earlier, but the superficial evidence is not clear, and in any case the age of the barrows is unknown.

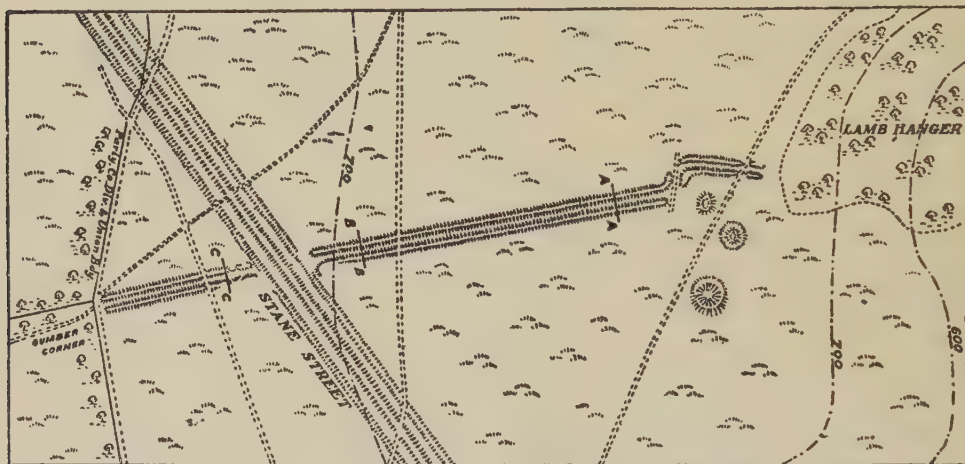


FIG. 4. GLATTING DOWN, SUSSEX  
Bivallate cross-ridge dyke, showing 'kink' and Stane Street crossing dyke

— SCALE OF FEET —  
100 0 1 2 3 4 5 6 7 8 9 10 11 12 1300

From E. and E. C. Curwen, 'Covered ways on the Sussex Downs'  
By permission of the Sussex Archaeological Society

The evidence that the East Yorkshire multiple dykes are later than the Bronze Age barrows appears to be conclusive; and the long bivallate ditches on Salisbury Plain have in several instances been proved to be not earlier than the Iron Age.

The case for connecting this type of linear earthwork with cattle is a strong one. So much attention has been directed of late years to the agricultural field-system of Celtic times, that study of the pastoral

<sup>9</sup> *Covered Ways*, pp. 62 et seq.



## CROSS-DYKES

conditions has perhaps been somewhat neglected. But the more we know of the high development of cultivation and agricultural settlements the more we must recognize that the need for cattle management must have advanced with it. The free roving conditions of pastoral nomad life were no longer tolerable : paths to which cattle could be confined for the protection of the crops, enclosures and regular stations for the cattle industry and trade must have been ultimately needed. For the first purpose the cross-country bivallates seem well adapted : their narrow floor 4 feet below the surface, their confining banks, their radiation from settlements, their leading to ponds, their skirting cultivation areas, their turns and twists at field corners all fit in. But can the bivallate cross-ridge dykes be brought into the picture ? Their position between the steepest scarps, where there are far greater stretches of Down with gentler slopes, and numerous low dips and saddles which might equally well have been chosen, puts it to my mind out of the question that they should have been constructed as roads or covered-ways across the ridges, either for cattle or for men : nor has any explanation been offered for several such covered-ways existing side by side.

I suggest that these short ditches were constructed not for cattle-ways but for cattle-pens—the single ones perhaps for confining the travelling or other cattle at night, and the compound ones for collecting, sorting, and marking the cattle of the community. If such were the case most of their peculiarities—the diverging ends, the paths up the slopes, the ‘kinks’ and the covering protective banks—would be readily explained ; while their position and the presence of anomalous defensive banks amongst them can be explained as being due to the universal tendency for peaceful establishments to grow out of others on sites originally chosen for their strength.

The question is, are they suited for such a purpose ? Unusual though such a construction may seem to us, I think they are. Consider their original section. A ditch, 1 or 2 feet broad at the bottom, sunk 4 feet deep in the ground, with steep sides and with banks rising some 6 to 10 feet or more above the floor, with or without a fence at the top, would be a pretty safe place to stand even the active little cattle of the day. They would have small chance of turning round or stampeding if the ends were closed by some sort of gate or fence, and would be under the control of a man standing on the bank. To a pit-dweller used to working in earth such a construction would not be unnatural, and one has only to look at the earthworks in Rewell Woods near

## ANTIQUITY

Arundel, discovered and planned by the Curwens,<sup>10</sup> which I believe to have been a cattle compound, to realize the extent to which digging has been carried. Here there are over 3 miles of ditches remaining, included in an area of over 80 acres, all of which appear to have been bivallate, except a piece of the enclosing entrenchment.

Apart from its use as a highway, the open ridge of the South Downs must have been of great importance in pastoral times. Where these compound systems of bivallate ditches occur, the woods and scrub come not only to its foot on the north, but clothe the long slope of 4 or 5 miles stretching down to the south before reaching the flat tertiary forest-covered plain of the coastal districts. Beechwoods now cover much of the slopes, and thicker woods fill the valleys. The bare chalk spurs such as Bow Hill, Fernbeds Down and Bexley Bushes have their slight earthwork enclosures, doubtless pastoral settlements such as we find in Wilts and Dorset ; and the main ridge has earthworks along its line. The half-wild cattle belonging to each community must have wandered free over the unenclosed slopes and, as in all pastoral countries at the present day, there must have been a great rounding up at least once a year for the sorting and marking of the young stock, gathering in the milch cows for the support of the settlement, the supply of trade and other purposes. One can picture the driving in of the cattle along the well-worn tracks converging on the open hill-tops, where alone they could be mustered and dealt with, much as cattle are brought in today in the wide Estancias of South America, the banked up or wattled 'slunway' taking the place of wire. The end of the journey at an Argentine station is made in a 'cattle-race' consisting of sloping wooden walls with a very narrow bottom along which the cattle are driven in single file without a chance of turning round. Across the end of such a race is a bridge on which a man stands and operates a swing gate, sorting the beasts as they come towards him and turning them right or left into their appointed paths.

We have on these ridges ditches capable of confining cattle, pathways sloping up to them, 'kinks' which suggest gateways, and covering banks which suggest enclosure. We have a right to ascribe to their makers skill in the construction of wattle fences, gates, swing bars and woodwork of all kinds. We have only to supply a little ingenuity and imagination to reconstruct the whole scene.

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<sup>10</sup> *Sussex Archaeological Collections* 1920, vol. LXI, 20-30. [These will appear on the forthcoming revised edition of the 6-inch Ordnance Map.—EDITOR].



# A Pedigree of Anglian Crosses

by W. G. COLLINGWOOD

**S**CULPTURED stone crosses are simply a local and temporary fashion in gravestones. They came into vogue when the northern English church-builders learned stone-carving, and their use spread in every direction through the British Isles, where it lingered on the Celtic fringe after the fashion had gone out in its first home, and afterwards it wandered farther afield. But by Anglian crosses is meant here only those set up under the influence of the Angles of Northern England before the Danish invasion, and those in which the same influence survived in parts not dominated by Danish or Norse settlement.

Before the invention of the Anglian cross, carved with ornament and showing a cross-head standing free against the sky, there were pillars and obelisks marked with the sign of the chrism or the cross. These are not properly crosses, as the word is used here, though out of them, no doubt, the Anglian type developed. The date of this invention of the free standing stone cross is not yet generally accepted : the older antiquaries held that it was in the 7th century and well before the time of Bede. The main reason for this belief was that on the best-known of the series, that of Bewcastle, the names inscribed are of persons who lived about 670-680 : and the dating of all the rest has been fixed mainly by the attribution of this one cross to that period and by little else in the way of argument. In this paper it is proposed to shift this date of the beginning of Northumbrian crosses about 60 years later, *i.e.*, 740, instead of 680 or so, which has been the popular date for the start of Anglian monumental sculpture.

It seems a trifling point, but that judgment may not go in default, and since explanation is needed, I should like to set down, in as few words as possible, what I conceive to be the development and chronology of our early Christian monuments. For figures I refer to my *Northumbrian Crosses* or any other illustrations, hoping that the reader is not unacquainted with the principal examples.

Though the later works of this movement are multitudinous, those strictly to be referred to the period before the Danish invasion

## ANTIQUITY

are not so very many. Of the pure Anglian schools (740-900) there are not more than about 50 pieces known, reckoning as units those which can be restored by putting together existing fragments. No doubt this does not represent the whole output of the age, but these monuments were costly and presumably therefore rare. Their remains have been so long known that further description is hardly necessary in most cases.

This tends to indicate that we now have seen the greater part of the original series, enough to reason upon. Of late, new finds have represented chiefly the age following after the Anglian: most of the new stones are in a fragmentary condition and have been unnoticed for that reason. Some few of the class may still turn up, but if we can place what are known into a series we have solved the problem provisionally, and more cannot be expected of archaeology, unhelped by written history. But to do less is unscientific, and we must take them as a class interrelated, not separately originated—each a *lusus naturae*, as the ancients regarded fossils.

It is unlikely that our crosses were independent of parentage and made by different workmen, mostly foreign, the artists or groups of men creating, in every case, a new design; because that would imply their importation or imitation from a foreign model, which does not exist. If the Reculver fragment is Saxon and carved about 670 (Clapham, *English Romanesque Architecture*, p. 62 and pl. 20), its unique character is such that we cannot bring it into our series. It would be simply a 'boulder in the formation', an intrusion without parents and without progeny. Its lines and relief are far more subtle and accomplished than anything else in Anglo-Saxon art of anywhere near that time. It cannot be called a cross among those we know; if it was such a work it is extraordinary that it should have left no impression on the art of the 7th century or for 100 years later.

All the crosses before the time of Bede—and there were no doubt many—seem to have been of the kind set up by Oswald at Heavenfield or by the Irish at Iona, that is to say of wood; or else simple columns of stone, unornamented and rude. The reason for this generalization is that we have no convincing allusion to sculptured stone as monumental art in that period. Those mentioned by William of Malmesbury and others are dated by modern writers only from the personal names attached to them. Several known examples, similarly attributed, can be explained as belated memorials of persons long dead, but for some reason commemorated by the piety of after ages. Such must have



## A PEDIGREE OF ANGLIAN CROSSES

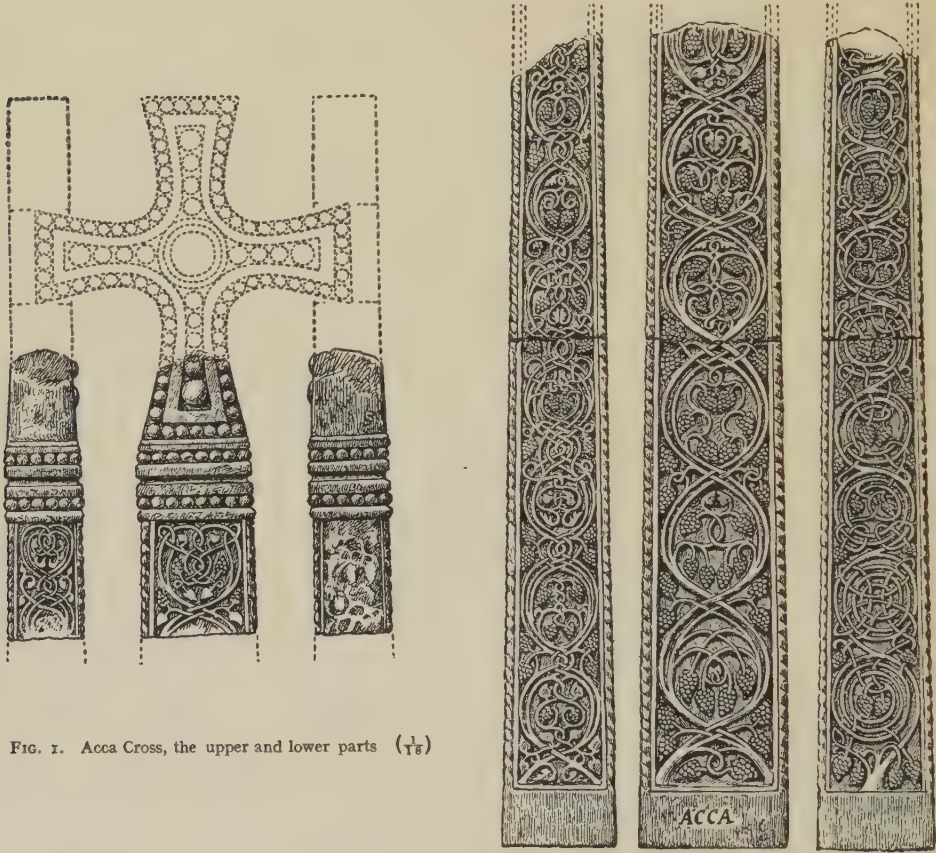
been the cross to Paulinus at Dewsbury, on which Leland saw the saint's name ; for long after his flight from Yorkshire in 631 Dewsbury was a place where no such monument could possibly have been erected. Such also was the Hackness cross to the unknown Ethelburga, obviously a belated memorial. Such, I think, was the Bewcastle cross, perhaps a hundred years later than the persons it appears to name ; I have suggested an explanation of this in *Northumbrian Crosses*, p. 116 ff. And the two great monuments at Glastonbury described by William of Malmesbury (I, 20)—one 26 feet high in four storeys, naming men who died at various dates before 705, and the other 28 feet high with five storeys—need not be taken as carved at the date of the persons recorded upon them. These monuments are also mentioned by Wendover as near 'Arthur's' supposed tomb. I don't doubt their existence, nor that of Arthur's tomb, whatever it was, but only their date, as contemporary with the names read on them. They are no evidence of great stone crosses before Bede's time.

A curious example of a work of art standing out of the Anglian series is the slab which can be restored from pieces at Hexham church and in Durham cathedral library, representing a child as archer with animals and birds among tendrils of vine. It is said by some that it cannot be Roman because there is nothing to match it in the region of the Wall. Rivoira classed it as Anglian, and others follow him. Its exact find-spot is unknown, but it certainly is quite unlike any Anglian work in the carving and design. It is more like the stone with birds and wreath (figured *Northumbrian Crosses*, p. 22) at Hexham church, which is known to have come from the crypt, and therefore cannot possibly be part of an Anglian cross ; it was built in at that place as valueless by Wilfrid's men, like the well-known Roman horseman discovered in the foundations. This must have been about 678, implying that up to then decorative carving from Corstopitum or other Roman sites was regarded as rubbish, neither material nor model of ornament, though used in Wilfrid's building merely to fill up gaps, not to be seen.

ACCA CROSS (fig. 1). But Acca (bishop of Hexham 709-732 ; d. 740) brought foreign art to Hexham and both enlarged and embellished Wilfrid's church (*Bede's History*, v, 20). His additions, I think, can be distinguished from Wilfrid's rudimentary work of a generation earlier (see *Arch. Aeliana*, series 4, vol. I, 65 ff.). At Hexham he must have had a body of workmen able to do stonecarving—at any rate

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they were living there while he was bishop, and when he died after eight years' exile some skilled foreign artists, probably Italian—we have no trace of Orientals—may have set up the cross now at Durham to his memory at Hexham. This is thought to be Acca's monument because the parts found at and near his grave tally with



Symeon of Durham's description of the place as it was in the 12th century (*Hist. Regum*, s.a. 740). The name of Acca does not appear on this stone as we have it and there may be some doubt. But even if this is not Acca's, nor of his date, there is that about it which makes it the parent of Anglian crosses.

It does not stand apart from the Anglian crosses, like the Reculver stone or the Hexham archer, for it is obviously of the Hexham series.



## A PEDIGREE OF ANGLIAN CROSSES

It is the one thing which is thoroughly un-English and yet cannot be assigned to any definite foreign origin. On the one hand this Acca cross is not native English : no Englishman at any time in the Anglo-Saxon age conceived such a set of artistic and delicate lines. There is nothing in our ancestral art to match it for variety and beauty as a set of variations on a single theme. All of the 24 remaining loops (some being lost) are perfectly harmonious and carry out the same motive without the introduction of any contrast but what is in keeping with the fundamental pattern. It is not on the other hand a foreign import or imitation. Monuments like the Roman pillar of Igel (near Trier ; 21 metres high and carved on four flat sides, *c.* 200 A.D.) cannot have given suggestions, and the cross-head is wanting in Asiatic gravestones. No Oriental or West European monument shows such a head, very simply planned and suggesting that this addition to an elaborate shaft was a novelty and an experiment, designed by an innovator. This head betrays a wooden construction, with its superimposed cross edged by mere pellets, imitating nail-heads. The cross is not in any way influenced by any other known cross. This imitation of woodwork and the naturalism of its ornament show that it is early : a later designer would have fallen into one of the conventions already established. It has no real vine-leaves, showing that this motive, tempting to a southern artist, was not present in the mind of its creator. It has the Roman form of scroll, with junctions everywhere interwoven, not like the Syrian links of stiff lines. One loop is formed of a leafless knot of bare boughs, but this is not treated like later interlacing, as a patch of different character on the design, but in harmony with the rest so that it almost escapes notice. It is true that the detail is now worn in places, and difficult to follow : more than one interpretation of some parts is possible ; but the character of these forms cannot be mistaken. It is the unique design of a man working at Hexham before the Anglian crosses had received any conventional type which he had to follow.

### THE HEXHAM SCHOOL

From this Acca cross as an unapproachable model, and in more or less clumsy 'English' imitation of it, one can trace a definite style through examples of the Hexham and Lancaster schools. This is the 'pedigree' of the earlier Anglian crosses—their connexion, linked in series, to which I now call attention. Why it happened to be transplanted to Lancaster it would be idle to ask, but that such was the fact the remains prove. I suppose workmen from Hexham, for some

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reason which we do not know, were invited to Lancaster abbey, of which we have traces in its monuments, but no literary evidence, and it must have existed as an important centre before the Viking age.

2. The Stamfordham shaft, perhaps originally at Hexham or at any rate by Hexham carvers, is the natural outcome of imitating Acca's cross. Joints were thickened, for which there was full authority in Roman work, and the whole is much simplified, but nothing is original or not derived from the model.

3. The great cross (Durham cathedral library iv) originally at the foot of Acca's grave, though not necessarily connected with that burial but merely its accidental neighbour, might almost be the work of the carver of the Stamfordham fragment. The differences are only in small detail of toothed leaves. But it must be noted that the lowest loop on the broad side, resembling the Acca cross in being a knot with no leaves or berries, shows its source. The work must be an imitation of Acca's cross.

4. Lancaster 1 is in the Hexham style, even in having a bare-boughed tangle, like the last ; but its joints do not thicken, and on the narrow side the boughs run in more flowing curves, not so tightly drawn.

5. At Kendal church is a small piece of a cross either from Lancaster or of Lancaster style, which means a distant origin in Hexham.

6. The Spital shaft is like Stamfordham, but one side has the figures of the Crucifixion added ; indeed it might almost be part of the Stamfordham shaft if it fitted the dimensions. This need not be much later than the date of Acca's death. An English carver set to imitate Acca's cross would produce something of this kind. No length of time is required for its 'evolution'.

The following are later but in the same tradition.

7. The 'Hardwini' cross at Lancaster is another of pure Hexham origin, though somewhat late.

8. At Heysham near Lancaster the 'doll's house' cross has scrolls of Hexham form, but its knotwork shows it to be of the 9th century, although of the same tradition.

9. The great cross at Heversham (fragmentary), in which beasts are introduced, is evidently of the Lancaster-Hexham school, but with new motives added.



## A PEDIGREE OF ANGLIAN CROSSES

10. The cross which I restore (*Northumbrian Crosses*, fig. 42) from Mr Lockhart's piece of a head, the shaft in Durham cathedral library (no. v) and the base in Hexham church, is plainly in the tradition of Hexham: its period is shown by its triquetra and the elaborate base with late knotwork which fits it.

11. The cross I put together from pieces at Simonburn has a Hexham scroll with later developments in the way of birds and flowers, but still of Hexham origin.

12, 13. And at Nunnykirk and Falstone are fragments of late development, much varied from the original source, which derives ultimately from the same motives, in the scrolls of Hexham.

These last six are of various dates in the age following Acca's death, but by workmen deriving inspiration from Hexham conditions.

### HEXHAM'S RIVAL: THE SECONDARY SCHOOL OF NORTHUMBRIA

At some early date a rival style sprang up which perhaps may be attributed to the workmen of Benedict Biscop's foundations in county Durham, though the chief reason for the guess is that Benedict's foundations were the known rivals to Hexham. This style improved on the simple scroll by adding birds and beasts, and plaits contrasting with them—in fact, using in stone the stock manuscript illumination patterns. It may well be asked, why should these not be dated earlier than the Hexham style? The answer is that if it were so, why the pure Hexham style at all? Why did not Acca's men or their direct successors introduce a figure or a plait into their elaborate designs? They were evidently meant to be as complete and as rich as their art went; in size and labour spent on the work nothing was spared, but these motives which would have added so much to the interest were absent. Would the early carvers of Hexham have refused the figure-ornament and knotwork which might have made their crosses more interesting if they had known them for possible subjects, as their followers, even at Hexham, did use them later?

Of monuments not directly in the Hexham tradition, the most primitive in style is (14) the cross at St. Andrews, Auckland, with its great base, now in fragments. The figure-drawing is nearest to that of St. Cuthbert's coffin, and the execution is elementary; the detail laborious and naturalistic. The base has on one side three saints in very low relief, and on the other side one saint in higher relief but very

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flat, all ungainly and with big clumsy hands, in the conventional Anglo-Saxon attitudes. The shaft has all the appearance of naïve beginner's art ; and it cannot be understood but as the work of one who, having seen or heard of Acca's cross, tried to surpass it with fresh motives, introducing figures and bird-scrolls but no plaitwork. Compared with the other great figure-crosses, it is obviously early. The dedication to

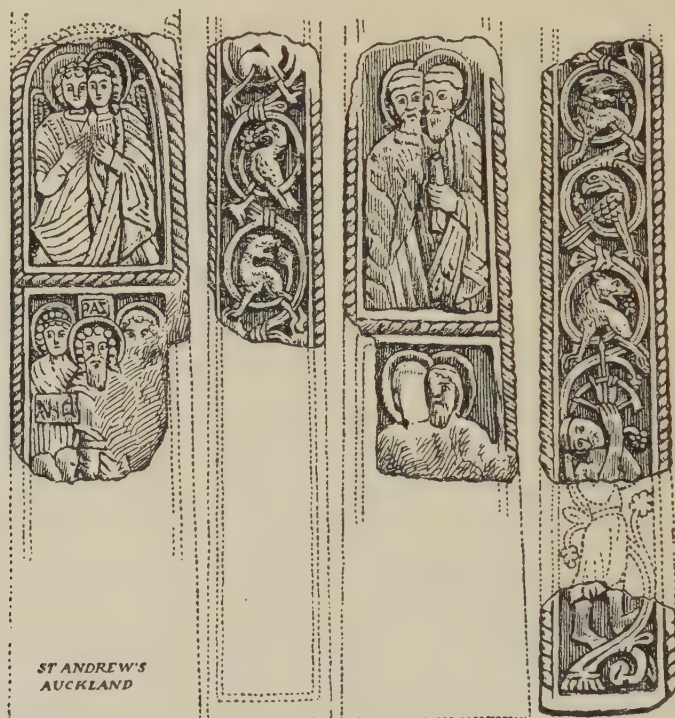


FIG. 2. Cross-shaft, Auckland ( $\frac{1}{12}$ )

St. Andrew, if ancient, connects the site with Hexham, the home of the St. Andrew cult : and it might therefore possibly be the work of one of Acca's group of men, independent of those who made his cross. I take it as the first of the Anglian figure and bird-scroll crosses, made not long after Acca's, but for reasons already given, certainly not earlier. Put the Acca cross at 740-745 and this soon after, say 750-755, and the others might follow rapidly when once the vogue set in. There is no need to postulate long time for development, if the setting up of crosses became a fashion in this limited area of Northumbria. (Fig. 2).





FIG. 3. The Otley Cross : shaded parts original, outlined parts restored (1/2)

## ANTIQUITY

15. A good while afterwards, because time must be allowed for the maturity of its technique compared with the last (14), is Otley Angel-cross. It is comparatively late, one of the proofs being that it had spaces left for lettering, as very early crosses had not. The animals are drawn with more freedom than at Auckland, and the figures show very much more accomplishment, though it contains no panel of plait and therefore has no sign of the 9th century. I put this about 800 and suggest it as the work of someone following Hexham scroll traditions but not working at Hexham. (Fig. 3).

16. The cross once at Easby appears to be later. It has elaborate plaits, and scrolls passing into plaits in a very advanced fashion—an eagle, finely drawn in scroll-work, and a *majestas* of almost classic completeness and accomplishment. There is nothing more ripe in cross-sculpture. It is the best kind of work before the tide turns to degeneracy approaching the middle of the 9th century. The Easby carvers seem to have been trained in a school akin to that of the Bewcastle designers, wherever that was; and the work is not pure Hexham, though it must be partly of that tradition. We may inquire where the school was that could produce it, and I venture to suggest that the artists were trained at Hoddam.

### THE HODDAM SCHOOL

There must have been a great workshop of sculpture at Hoddam, sprung from Hexham. From Hoddam itself are four examples (figs. 4, 5; not now there) of Anglian stone-carving of this best period, as well as others of much later style, showing subsequent continuance of output throughout the 10th century. Also Ruthwell is near the abbey and no doubt employed Hoddam men, and the Bewcastle cross stands only 25 miles away, in the same ancient district; there was nothing to prevent workmen from the one place working at the other. The Langbar stone, if intended for Bewcastle, seems to hint that the carving of that cross was locally done by men travelling to their job.

17. Of Hoddam remains, the stone with the 'doll's house' motive now at Edinburgh is somewhat uncouth and yet not very early as crosses go, because of the large space left for inscription—a trait of comparative lateness. It resembles the Anglian stone at Heysham, a work of the Lancaster group, with this 'doll's house' motive but with scrolls of distinctly Hexham origin and with rather late plaits, *i.e.*, plaits of 'easy' construction. These two sculptures must have been



## A PEDIGREE OF ANGLIAN CROSSES

made under the same influence, and hint that Hoddam was in connexion with Hexham, where the earlier scroll-type arose. That is to say, Hexham men taught Lancaster and directly or indirectly influenced Hoddam. (Fig. 4).



FIG. 4. The 'doll's house' cross from Hoddam ( $\frac{1}{12}$ )

18. Cross B of the three crossheads restored from Hoddam fragments (formerly at Luce) also has the open flowing scroll seen at Lancaster and Heversham, betraying the presence of some of the group of men who came originally from Hexham and worked both at Hoddam and Lancaster.

19, 20. But Hoddam heads A and C, while connected with the last by a central figure instead of a mere boss, are a new type; and the form of the cross-head ('spatuled') is that of Ruthwell as it was originally. (Fig. 5).

## ANTIQUITY

21. The Ruthwell cross can hardly have been made by artists other than those who worked at Hoddam. Question has been raised as to their origin: these examples, set side by side, settle it. The Ruthwell cross is not Hexham work, but derived in part from Hexham through the school of Hoddam at an interval of a couple of decades. (Fig. 6).

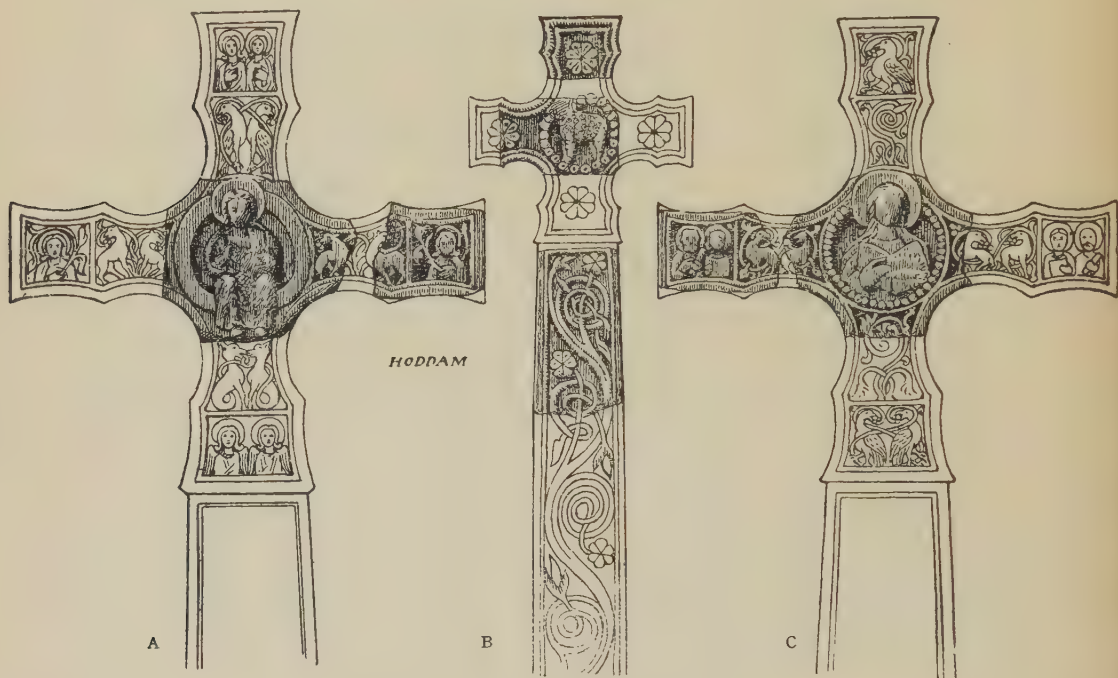


FIG. 5. The Luce cross-heads from Hoddam ( $\frac{1}{12}$ )

22. And if Ruthwell cross was made by Hoddam men, so must Bewcastle have been, only later—later because it is more mature and because at Ruthwell no spaces are provided for lettering, which was added after the cross was complete, while Bewcastle has (like the ‘doll’s house’) a big panel left blank for the purpose. In other ways it shows a much greater number of motives—plaits of advanced types, the chequers and the dial; while the scrolls, though more elaborate and very florid, are still ultimately of Hexham origin, as shown by their interlacing at the crossings of the loops. The Falconer, too, as A. S. Cook pointed out, is not likely to be before late 8th century. In fact,





FIG. 6. Ruthwell Cross ( $\frac{1}{3}$ )

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I do not see how it can be placed earlier than the end of that century ; it is a belated memorial to Alchfrith and his family (see *Northumbrian Crosses*, p. 116 f.). (Fig. 7).

This brings us down to about the beginning of the 9th century. In the past 60 years we have seen a small number of workmen at Hexham, Hoddam and Lancaster as their chief centres, and perhaps other places, bringing the art of the monument to such perfection as it attained in the early Anglian (pre-Viking) series of which we have the remains of less than 20 examples, datable before 800. (The numbering above includes 10-13 which are later pieces, though of Anglian style, and therefore included in the list). No doubt once there were more, but I cannot trace remains of more than these to attribute to this early period, 740-800. That being the case, the output and the progress of development were not too great for the time allowed. After about 800 we begin to see the effects of a somewhat different school, in work often on a smaller scale, highly refined and of florid design, some of which can be attributed to Ripon.

### THE SCHOOL OF RIPON

The pieces left now at Ripon are (A) the cross-head (fragment) delicately carved, with chevron border and triquetrae on one side, chevrons and 'lorgnette' (superimposed crosslet) on the other, and a knot at the end of the arm ; (B and C) two interlaced stones (built into a buttress at some late restoration) with 9th century knotwork, *i.e.*, parts of a cross-shaft ; and another (D) head of 10th or 11th century type. At Kirkby Hill is the impost of very fine plait and scroll, apparently from Ripon, but built into an 11th century doorway here, from a door of earlier date.

These seem to give a clue to the source of a number of 'lorgnette' and chevron crosses as deriving from Ripon, namely Northallerton, once an important monument (fig. 8); Hornby with the 'loaves and fishes', very delicate carving; Carlisle, a fragment of chevron head, less finely carved but having the Ripon motives; and another at Heysham, where the Lancaster-Hexham style had not a monopoly.

To judge by delicacy of carving, which is certainly a Ripon characteristic, a few may be added as works presumably of the school. The Otley dragon shaft with its late plait and creatures tail to tail; the Croft fragment, very refined in cutting but certainly late in the Anglian age by its plait and upside-down animals. Here we have perhaps the first examples of the tree scroll, along with a pretty scroll





FIG. 7. Bewcastle Cross ( $\frac{1}{12}$ )

## ANTIQUITY

distantly derived from Hexham and like that at Kendal on which three twigs meet inwards, almost forming 'late Celtic' trumpet pattern. Add to these the Dacre lion, also very delicate, with unusual key-pattern; and the two recumbent grave-slabs at Melsonby—these all are of mid-Yorkshire origin in the 9th century. To these crosses add the neighbouring and coeval stones at Tanfield, Wensley, West Witton, Wycliff, Gilling West ('lorgnette' head on a round shaft), all of which

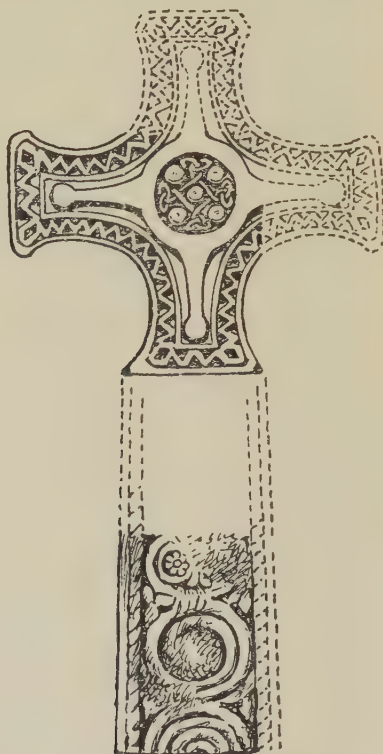


FIG. 8. Northallerton Cross ( $\frac{1}{12}$ )

are minor works. The great crosses of Masham and Cundall are connected with Croft by the grotesque drawing of their animals, not inelegant but heavy-necked. Masham has a great round shaft to which no doubt belonged the fragment of a large head (now in the church) of florid scrolls and knotwork: and the lower tier of the shaft faintly resembles Cundall (parts formerly in Aldborough Museum), in having the heavy-necked beasts. The beginning of this ungainly beast with



## A PEDIGREE OF ANGLIAN CROSSES

heavy neck is traceable in Ruthwell and Bewcastle crosses, enough to suggest affiliation of the (Hoddam) carvers with the subsequent (Ripon) school at no very long interval.

The Hovingham slab of the Annunciation and other figures under arches and a good bird-and-beast scroll is closely like the work at Masham and therefore of the Ripon school, early in the 9th century.

### LATER YORKSHIRE CROSSES

Hackness cross, by its late scroll and plaits, cannot date before the third quarter of the 9th century. Another indication of its lateness is the double outline to the confronted beasts, seen also at Crofton. They are in the mid-Yorkshire tradition, distantly descended from Ripon, which may have been the source of many minor groups of carvers, such as those who migrated north and after the Danish invasion worked in county Durham, Northumberland and the south of Scotland (Jedburgh, Closeburn and Thornhill in Dumfriesshire). (Fig. 9).

As a late work of a carver of the Ripon tradition (*i.e.*, end of 9th century) I should class the Ærswith cross at Collingham, which has one motive of Cundall distinctly repeated in the beast with head stooping down between upraised forelegs. And if so, the Apostles cross at Collingham, round-shafted when complete (like Masham, only smaller), is of similar date and tradition. The whole Ilkley school can be connected with it, as inspired from mid-Yorkshire though becoming an independent group. It was not foreign work or the importation of fresh ideas that produced the monuments of Ilkley, Dewsbury and Thornhill (Yorks). They derive in a straight line from the Yorkshire school.

At York itself we have only minor and later remains. Adhusa's cross (Hospitium 13) is a 'lorgnette' known to be from Ripon, but quite late. The head fragments (Hosp. 11, 12) are of fine late Anglian work and form. No. 11 is 11th century by its unornamented surface, and no. 12, inscribed 'Salve pro meritis' etc., is in that respect similar. So the 'Two Gentlemen' (Hosp. 9) have a coarse late scroll, and the stone (Hosp. 21) with the unusual knitting-stitch and long-necked beasts. All else at York is later still. In fact the great city of York does not seem to have had a monumental style of its own in early Anglian days: or if it had, the whole of such remains perished under the stress of the Danish attack. And yet it is hardly likely, considering what happened elsewhere, that no trace should be left, if there were any actual remains.



FIG. 9. Attempted restoration of the Hackness Cross ( $\gamma_2$ )

## A PEDIGREE OF ANGLIAN CROSSES

### OTHER LATE SCHOOLS

To return to Lancaster and the south. At Lancaster itself a school of carvers evidently flourished continuously up to the 11th century. Some of their work we see at Halton on the Lune, in the fragments of a tall cross with the flock of sheep and the Angel of Remembrance. This last motive is like one in the Otley Angel-cross, and it was copied in a second cross at Halton, on which the tree-scroll and the plaits are not earlier than the end of the 10th century. Another fragment betrays a curious overlap with the Ringerike style of the early 11th century, leading to the 'Sigurd' shaft which shows an extraordinary survival of the same old Anglian ornament blended with Norse of the age of King Knut. Thence onward we find, in Lancashire and far south—through Whalley and Sandbach, Bakewell and Sheffield, Eyam and Derbyshire generally, Northampton and Notts, and even to Cropthorne in Worcestershire—a series of remains, Yorkshire-Anglian in ultimate origin but Ringerike-Norse in character, which seem to be the descendants of this school in the early part of the 11th century—not independent creations but affiliated to the movement which had died out long before from its first home in Northumbria.

Parallel but distinguishable developments went on in southwest Yorkshire and in the north. In Yorkshire we see the school of Dewsbury and Thornhill growing out of Ilkley and therefore connected with the 9th century Yorkshire art, but specializing in plaitwork of a non-Danish type (at Thornhill), but certainly later than the Danish invasion. It is plain that when the Danes settled in Northumbria they settled only on the east of a line drawn roughly through Leeds north to south. There is no trace of the Danish Jellinge work (first half of 10th century) elsewhere, though it is common in that district; but the old kingdom of Elmet (roughly the West Riding) remained Anglian in culture until about 950 when the Norse settlers filtered in from the west, and Scandinavian design mixed with the older English which survived in many places; for example, in the neighbourhood of the Walton cross, where the main element is debased Anglian but tinged with Ringerike, not Jellinge character.

In the 10th century there were in the north two principal schools, that of Hoddam and that of county Durham. From the first came some late Anglian-style crosses in Cumberland, as at Irton, and in Dumfriesshire (Closeburn and Thornhill being of Hoddam style). From the second very many late works clearly descended from Hexham style (some already named at Simonburn, Falstone, Jedburgh) and



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south Scottish pieces (Abercorn and throughout Lothian), in districts where the old Anglian population still inhabited or descendants of the carvers driven out by the Danes still found employment.

These by slow gradations became Celticized, and the well-known Scottish crosses were the result of a movement begun far to the south, but changing character as it found itself in the new surroundings. Ultimately it became the Pictish school of the 11th and 12th centuries, parallel to the developments of that age in Wales, Cornwall and Ireland.

Viewed in this way we can see a perspective of development perfectly logical and flowing without a break from Acca's cross as progenitor. I confess that my pedigree has no room for the Reculver stone as Anglian of the age before 700, but I do not admit that the series presents difficulties. There is no undue hurrying of the development in taking 740 as the start, because for the first 60 years we have only to find room for a score of works.

One more remark arises from criticisms made by supporters of the earlier dating of these crosses. The idea that the political high-water-mark coincided with the zenith of art is an illusion; it is contradicted by the art-history of every style and school. It is after the maximum of national energy has been displayed that art is at its highest, and that was the case in the 8th, not the 7th century, in Anglian Northumbria. It was not in the time of Oswy's (Oswiu's) victories, still less in that of Oswald, that arts and crafts flourished. There was then enough to do in the struggle for life. It was at the time when bishop Albert was building and Alcuin was teaching, even though the dynastic rivals were disputing the right to rule and the Vikings were beginning to nibble at the fringe of the kingdom. The bulk of Northumbria was prosperous and wealthy till long past the time of Bede, and it was then that refinement and the interest in luxury and arts began to show their effects. Until the turn of the 7th to the 8th century, the Anglians were rough and rude; civilization came late to people who in 600 were boors, and in 500 barbarous. The outburst of cross-carving could not have taken place so early as the life of Alchfrith, when Wilfrid was the pioneer of culture in a rough borderland such as Northumbria was during his reign. The attempt to date the rise of art to this age breaks down when the circumstances are better understood, and that is why the present writer has been gradually and reluctantly forced to modify the views in which he was brought up by teachers of a past generation, who dated Bewcastle cross to about 680; that is why he is now led to place it at the more reasonable date of a century later.

# This Fieldwork

by R. E. M. WHEELER

‘IT is better to dig than to dance’, said St. Augustine. This sober choice between two difficult alternatives is one which, I freely confess, had always appealed to me as that of a balanced and discerning mind. Today I am a little shaken in my confidence. It has been brought home to me, with something of a shock, that this *obiter dictum* might in a certain sense be cited as a justification of the (to my thinking) distorted outlook of the modern archaeologist. At the present moment, for good or for ill, digging has, it seems, become to a large extent the basis of archaeological research. I say ‘for good or for ill’ advisedly. To those of us who may be classed as Antiquaries of the Older Generation, there is something indecorous, almost irreverent, in all this hot and untidy pick-and-shovel work. It is so hearty, so vocal, so undergraduate. Besides, it is so easy. Next to committing a murder, the easiest step to fame nowadays is to dig up a potsherd. And the more ignorant the excavator, the greater the kudos. ‘Potsherd dated 543 B.C. found in Kent’ may not be a hundred-per-cent. news-item; but ‘Mystery potsherd in Medway midden: experts baffled’ is a main-page headline, sure thing. It is hardly to be wondered that, in our sound-proof studies, surrounded by our genealogies, our heraldry and our armour, we slippered veterans move uneasily in our chairs. It is as though a fist had been thrust rudely through our window-pane and let a draught of cold, oxygen-laden air into our hitherto inviolate *sancta*.

However, times change and other antiquaries change with them. Fieldwork has presumably come to stay. As I pen my plaint, I have in front of me two volumes, one French, the other German. The former is entitled *Manuel de recherches préhistoriques*, and is a revised and largely rewritten edition (dated 1929) of a work originally prepared in 1905 by the Société Préhistorique Française.<sup>1</sup> It is associated with the names of E. Hue, Marcel Baudouin, Breuil, Coutier, Franchet, A. de Mortillet and others, and may be regarded therefore as the authoritative pronouncement of French field-archaeology. As the

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<sup>1</sup>Published by Alfred Costes, Paris, 1929. 198 small and sometimes obscure illustrations.

## ANTIQUITY

writer of the preface remarks, 'this Guide will, we are convinced, render the utmost service to beginners desirous of profiting by the experience of their seniors, and of learning in the school of the modern pioneers of the true Prehistoric Science'. Let us for a moment put aside our Papworth and (re-adjusting our cushions) attempt to follow the blazed trail.

In the first chapter we pick up our tools. These include the following: crow-bar, boxes, candles, compass, chisels, notebooks, lifting-jacks, hooks, rope-ladder, string, labels, axe, acetylene lamp, marine lantern, hammer, measuring-rod, blotting paper, packing-paper, cotton-wool, picks (various), pick-hammer, pincers, special knapsack, silicate of potassium, drill, sounding-iron, glass tubes, water-colours, etc., etc. In chapter II we survey the landscape in search of likely spots, and in chapter III, having selected our region, we proceed to put the natives through the Third Degree. Here we first strike that note of caution which recurs like the drone of the bagpipes throughout the book. 'It is prudent not always to trust one's local informants in the matter of an excavation, a *depôt* or a discovery; their information may be false either through ill-will or because the informant desires to retain his knowledge in order that he may exploit it in his own interests'. Nevertheless, tact and persistence will win through, and the time will arrive when we shall have to approach the landowner. This should be done, 'either in the presence of witnesses or in writing'. More caution! Model 'agreements' are printed to facilitate these arduous and tricky negotiations, and success in the preliminaries is at last achieved. Now for preparations on the site. But oh, what perils do environ! You may fence-in your site with the utmost care; yet 'in spite of all these precautions, one must expect to be robbed, happy indeed if the outrage is only the outcome of the cupidity of a simple workman'. It is little to be wondered at, therefore, that, in engaging our simple workmen, we 'must enquire, before all, into their manual capacity and their honesty'. Remember, too, that '*the best way to ensure the honesty of your workmen is not to leave them a minute*' (author's italics). This affectionate get-together spirit must surely turn a French excavation-party into one great happy family.

Follows a chapter of some interest on the French laws relating to the disposal of objects found on private or on State property—a long and complicated business which need not here retard our progress. Then, before getting down to the actual digging, we are instructed in detail in the all-important task of recording our potential discoveries.



## THIS FIELDWORK

We begin, quite properly, by marking the site on our maps. We then prepare photographs, sketches and water-colours of the site. Measurements are important: but, since indications written on the picture detract from its artistic beauty (*beauté artistique*), reserve a large enough margin for this purpose. From artistry we turn to geology and thence to the collection of pertinent local folklore. For the latter, it is sad to reflect, we must turn to 'the old people, because they have preserved traditions more faithfully, whilst the young people sometimes ignore them completely'. If there are local superstitious rites, we 'must, if possible, take part in them in order to render an exact account'.

Eventually we approach more nearly to our muttons. Much sound advice is given in regard to the working-record of the excavations. Omit nothing—'note all the incidents of the excavation'. Even so, you cannot be sure that in the end anyone will believe you. If, therefore, there are witnesses of your discoveries, 'beg them to sign the report of every important find'. Take no risks. And, not least, do not forget that pirates are waiting eagerly to secure for themselves the credit of your discoveries. If you cannot publish a note on these at once, you are recommended to deposit an account immediately, in an envelope sealed with five seals, at the Academy of Sciences or the Academy of Inscriptions. Thus only can priority be assured and honours given where honour is due.

But we are not yet sufficiently qualified to sally forth into the great open spaces. In successive chapters we learn how to use a map and compass and to make a traverse; how to preserve objects after discovery (a pretty good chapter, but sketchy in the matter of metalwork); how to make models; and how to photograph. The chapter on photography would have been more illuminating without its illustrations. These consist of a series of foggy blurs alleged to represent megalithic monuments, *in each case* (so far as visible) *without scale*, although the text has all the right ideas on this point. And once again we meet the ghost at the party. 'Be sure to take a photograph at the *actual* moment of any sensational discovery, *including in it the persons present as witnesses*'. The italics are again the author's. How closely akin are duplicity and simplicity! It is without surprise that we are next confronted by a whole chapter on 'the authenticity of prehistoric objects', stimulated in some degree by the Glozel hoax and contributed partly by M. Champion, one of the exposers of that hoax. Chapters on the human skeleton, on craniometry, on pottery (quite useless), and on 'the installation and arrangement of collections' (a matter on

which France has not a great deal to tell the world) prolong our suspense, until we begin to suspect that our veteran pioneers have lost the way. Our suspicions are confirmed when we suddenly find ourselves entangled in the jungle of an irrelevant 67-page treatise on the whole of French prehistoric archaeology. The obstacle is a serious one, but if we persevere we eventually win our reward. On page 279 appears the blessed word 'stratigraphy', and we at last come to grips with our subject. The excavation of a cave is described at length and with much reason, for in this department of fieldwork the French have won their spurs. And there are points in the section on the exploration of chambered tombs, although in this matter the standards set in this country by Mr W. J. Hemp have rarely, if ever, been approached in France. But for the rest, even from the study-window it is easy to see that France has little to offer. An entirely unhelpful section on the excavation of hill-forts merely affirms vaguely that 'the excavation of a fortified enclosure should be carried out with the greatest care and the greatest observance of method'. What that method should be is not indicated. Another section, on the investigation of tumuli, advocates a system of pseudo-scientific mutilation that has been obsolete since the days of Pitt-Rivers. The whole book is a shoddy and pitiful display for the combined talent of France. We are left throughout with the impression that, at the best, the captain is polishing the brass-work and arguing with the cook instead of navigating the ship. It is inconsequent, directionless, and—un-masculine. Wake up France!

With this trumpet-call, we turn on our cushions more hopefully towards the German volume. Superficially it might seem to be the equivalent of the French handbook, for it is entitled *Grundfragen der Urgeschichts Forschung*.<sup>2</sup> A glance within, however, reveals a different world. Here at last is unity of command and definition of purpose. The word Method occurs significantly no less than three times on the first page, and all is indeed set forth in disciplined array. We stand rigidly to attention whilst the human sciences—Anthropology, Philology, Archaeology—goose-step past us. This, we feel, is as it should be. The author, Dr K. H. Jacob-Friesen, is the director of the Hannover Provincial Museum, and his book is an orderly museum-book. In it the multitudinous and inexact science of humanity is given simplicity and precision, is epitomized, labelled and docketed for easy reference.

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<sup>2</sup>Published by the Helwingsche Verlagsbuchhandlung, Hannover, 1928. 240 pages, 18 illustrations, mostly small distribution-maps. Mk. 20.50.

## THIS FIELDWORK

The epitome is overwhelmingly Teutonic, as such epitomes are apt to be ; but the author occasionally breaks his frontiers—is aware, for example, that a certain *Crawford* has plotted archaeological distributions in the Wash-Busen, and that one *Cyrrill Fox* has produced some instructive maps of Cambridgeshire. We may describe the book, therefore, as reasonably comprehensive, and may at the same time congratulate the author (and ourselves) on its conciseness. In these and other respects his work has many merits that are absent from its French contemporary.

But in detail, the two handbooks are not comparable. Dr Jacob-Friesen's underlying purpose is, perhaps, definition rather than detailed instruction. He sets the three sciences side by side, summarizes successively the main contributions of the more notable (German) scholars in each of them, tabulating their work by subject or by method ; and finally attempts to determine the proper scope and significance of each science, with a view to an ultimately closer co-operation between them. All this has a rather theoretical and abstract tendency, and the whole book is definitely of the arm-chair. Nevertheless, not a little of the incidental matter is of relevant interest. The section dealing with archaeology ('Kulturhistorische Forschungen') includes subsections on Distribution, Morphology, Chronology and Method (touching lightly, very lightly, on fieldwork), together with resumés of theories relating specifically to the early history or prehistory of the 'Indo-Germans', the Germans, the Celts and the Slavs. All this is very succinct and, up to a point, informative. One is left with the general impression that the book is useful. But useful to whom ? It is difficult to say. Perhaps to university honours-students, or to museum-curators ; anyway, to people who are generally too busy acquiring information to bother unnecessarily about education. Be it repeated, however, that the book is, of its kind, a good one and pleasantly produced.

And now we have suffered long enough the elbows of those jostling archaeologists whose unseemly boast is that they labour 'à la manière du Géologue et du Zoologiste, c'est-à-dire à la façon des Naturalistes'. Forsooth ! It is but a step to that knowing scientist who has computed that Man is but five-shillings' worth of chemicals. Let us gather our coat-tails about us and return thankfully to our brass-rubbings.



# Museums Old and New: some personal impressions

by O. G. S. CRAWFORD

**M**OST readers of *ANTIQUITY* will have heard of the Pergamon Museum at Berlin ; its opening was the chief event of the centenary celebrations of the German Archaeological Institute (see Mr R. G. Collingwood's note in *ANTIQUITY*, 1929, III, 339). Those, however, who have not actually seen it can hardly realize what an outstanding achievement that museum already is, even in its present unfinished state ; nor perhaps do they know that the Pergamon reconstitutions do not stand alone. There are similar reconstitutions of other classical buildings and also of Middle Eastern remains. About five large rooms are at present opened, and their contents represent the high-water-mark of museum-craft in Europe,<sup>1</sup> and are an object-lesson to the world ; but to me their significance seemed even deeper, marking the triumphal emergence of a new craft. I have seen many museums, but here for the first time in my life I felt completely satisfied. The ideal aimed at seemed to have been achieved ; there was nothing to find fault with. Here at last was a thoroughly honest attempt to reconstruct ancient masterpieces of architecture, and to exhibit them without irrelevant distractions. The technique of exhibition achieves its end through simplicity and common sense. The lighting is admirable ; the floors of marble slabs (pink in the Altar-room and white or grey elsewhere) are thoroughly pleasing ; the walls are delightfully bare, with nothing but the briefest of labels carved in admirable lettering. The effect of the whole is over-whelming.

Why ?

Because for once one is put through direct to the past with neither interference from the operator nor atmospherics from the environment. Here, one feels, the authorities are trying to obscure themselves. They

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<sup>1</sup> I am concerned only with Europe in this essay. Having never been to America I can say nothing at first hand about the splendid museums there.





THE PERGAMON ALTAR, AS RESTORED  
*Ph. Pergamon Museum, Berlin*





PERGAMON MUSEUM, BERLIN  
Betho



## MUSEUMS OLD AND NEW

want one to appreciate a work of art as its creator intended ; and that after all is how one approaches the past nowadays. One had come across valiant attempts of museum curators, faint but pursuing an ideal, but never success on so magnificent a scale. To have conceived and been able to execute a design like this betokens a great artist.

The whole setting is typical of the modern attitude, as I have said. Nowadays whether we are interested in prehistoric man, or the South Sea Islanders, or medieval serfs, or Tuscan artists, we endeavour to understand them ; we try and find out how they looked at the world, how they dealt with the fundamental problems, and the minor problems, of existence. In a world of ideal Museum Directors and overflowing treasures we might expect ideal museums. In the world as it is we seldom find them ; and it is most significant that the brightest example should have been created by one of the youngest republics in the darkest hour of its existence.

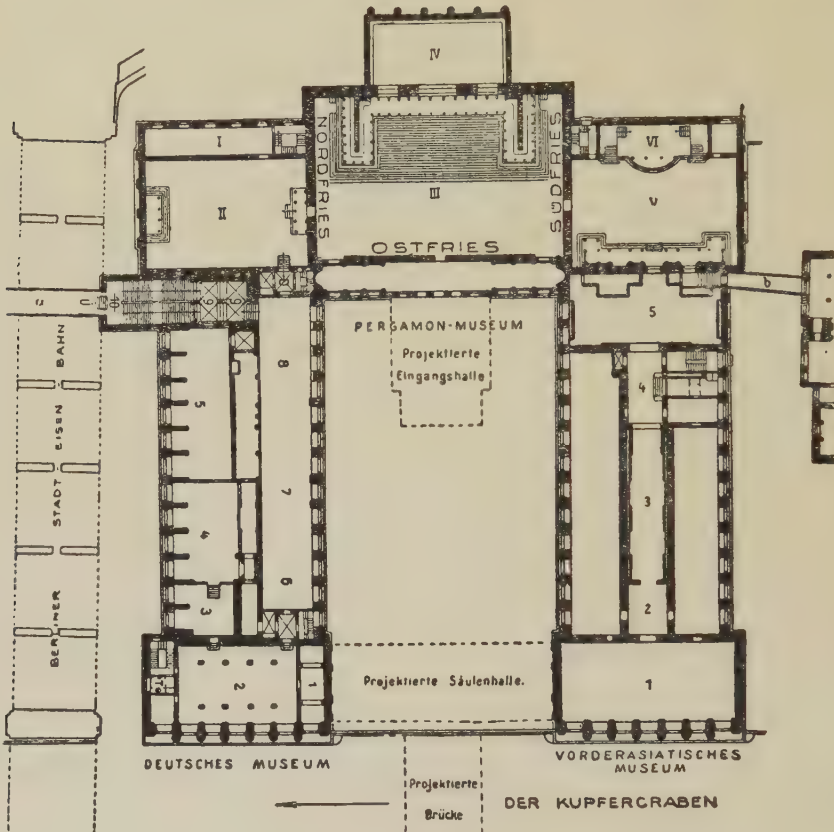
Formerly, of course, the view-point was quite different. The 'great ones' of the world used the masterpieces of antiquity as an offset to their own magnificence ; and interest in humbler things had its roots mainly in a Pharisaic smugness, contemplating the heathen in his blindness. We have got rid of that, but we still do not realize that the conversion of palaces into museums, though certainly in one sense a step in the right direction, was still only the first step. Palaces are themselves relics of the past ; the purposes for which they were built and the pretentious arrogance of their whole design are both utterly foreign to the spirit informing, for example, the Pergamon Museum. This new spirit, which is simply a manifestation of the scientific spirit, looks forward not backward, even when it is dealing with the past. It is not at home in palaces, the cast shells of a bygone régime. For guidance in action it looks not to tradition but to commonsense and the specific needs of the case. To the architect who is designing a museum, both imitation Gothic and imitation classical styles should be equally irrelevant and abhorrent. He has certain requirements of space and lighting and such like to carry out in a certain material (ferro-concrete usually), and that is all he need bother about.

In mere size the Pergamon Altar-room (pl. 1) is impressive. It is 47 by 30 metres in dimension, and the height is about 18 metres. The roof (seen in plate 1) is of glass set in an iron framework. Both walls and floor are absolutely bare, except for an inconspicuous desk where are sold some superb photographs (from which these illustrations



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are reproduced) and descriptive literature of both popular and learned character. No attempt is made to restore missing portions of the statuary, but such structural replacements as are necessary have been



Plan des Pergamonmuseums

- a. Übergang zum Kaiser-Friedrich-Museum
- b. Übergang zum Neuen und Alten Museum

added, in very good taste and in accordance with the best modern principles.

From the Altar-room (III) one passes to the Roman Hall (v), whose chief objects are the Markt-Tor of Miletus (plate III) and part of the upper portion of the Mausoleum of Carfinia; on the floor is a

## MUSEUMS OLD AND NEW

mosaic from a Roman house at Miletus (plate iv). The other objects include a couple of columns from Baalbek (seen in the middle of plate iii).

We pass out westwards into the Near Eastern Museum which occupies the southern wing and is divided into twelve rooms, of which only three are as yet open. We enter room 5 first, and see opposite us a reconstitution of the façade of the Parthian palace at Assur (about A.D. 200), the outcome of excavations conducted in 1911-12 by Dr Andrae. The whole effect is not unpleasing, though the architectural proportions might be better (plate v). It is obviously reminiscent of the Roman triumphal arch. As a link connecting the genuine old-Mesopotamian and the later Hellenistic architecture with Sassanian and early Mohammedan, it is obviously of great importance.

Thence we pass to another great achievement—the reconstruction of the Ishtar Gate at Babylon (plate vi) and of the Procession-way leading up to it (plate vii). Here the element of colour adds greatly to the effect produced, and the composition of the whole design is quite well done. A well-lit model (plate viii) and a plan hung above it enable one to grasp at once the place and function of these reconstructions in the ancient city of Babylon itself. A student who has spent half an hour or less here will go away with a clear-cut, permanent mental picture of one of the great monuments of antiquity. How many other museums achieve this? How many try to?

The last of the Berlin museums to be mentioned is the Tell Halaf museum (Franklinstrasse 6, Berlin-Charlottenburg). Here, in an old factory, are exhibited the sculptures and other objects excavated at Tell Halaf by Max Freiherr von Oppenheim, in the years 1911-13, 1927 and 1929. The building is admittedly unworthy of the treasures it contains, barbaric though their style may be; and one hopes that one day when prosperity returns to the world, they may find a better home. The indomitable pluck and perseverance which after 22 years of tribulation produced the Pergamon and Near Eastern museums will certainly triumph yet again over difficulties that may now seem almost insuperable: for nothing less than a whole new building, specially designed, will suffice.

In a future issue we may be able to give a further account of Tell Halaf. For the present, since we are concerned rather with the technique of exhibition than with the exhibits themselves, it must suffice to say that Tell Halaf is an ancient mound-city on the Khabur, midway between Aleppo and Mosul, lat.  $36^{\circ} 47' 40''$  N, long.  $40^{\circ} 6'$  E. It now

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lies within the mandated area of French Syria, and the sculptures here exhibited consist of both stone originals and plaster casts. In addition a few objects are exhibited, and also excellent plans of the site and photographs of the excavations.<sup>2</sup>

Let us now go to the most famous of all museums, the Louvre, and institute comparisons. Let me make it quite clear beforehand that I am criticizing *style*, *not persons*, least of all the distinguished conservators and their staff, to whom incidentally readers of ANTIQUITY have special cause to be grateful. Why is it that, of all big museums, the Louvre, which contains I suppose more masterpieces than any other building in the world, is the gloomiest and most depressing? Why does it seem to radiate an atmosphere of aloofness, almost of hostility?

Is it because it is built in a debased style, designed originally for the glorification of idle and useless people? Judged as a work of architecture by certain standards it is almost incredibly ugly, both outside and in. It may perhaps have been suited to the tawdry purpose for which it was made; but that is no justification, and in any case that purpose has ceased to convey any meaning to the world of today. The people who haunted its corridors have departed for ever; but here their atmosphere and traditions seem to linger.

Inside, several criticisms obtrude themselves. First, there are far too many exhibits, and they are crowded too closely together. One's attention is distracted, however hard one tries to concentrate. Secondly, the architecture conflicts with the exhibits. Thirdly, the defective lighting often prevents one from seeing the exhibits, or from seeing them to the best advantage. For instance the frieze of the

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<sup>2</sup> The following list of references was compiled at the museum :—

a. Ernst Herzfeld, *Arch. Mitteilungen aus Iran*. Band II, heft 3 (June 1930); chronological table giving synchronisms with Zenzirli, Carchemish, Boghaz Kiöi etc. in parallel columns. See also his Appendix in *Der Tell Halaf*, 1931, 225–233.

b. *Illustrated London News*, 25 Oct. and 1 Nov. 1930.

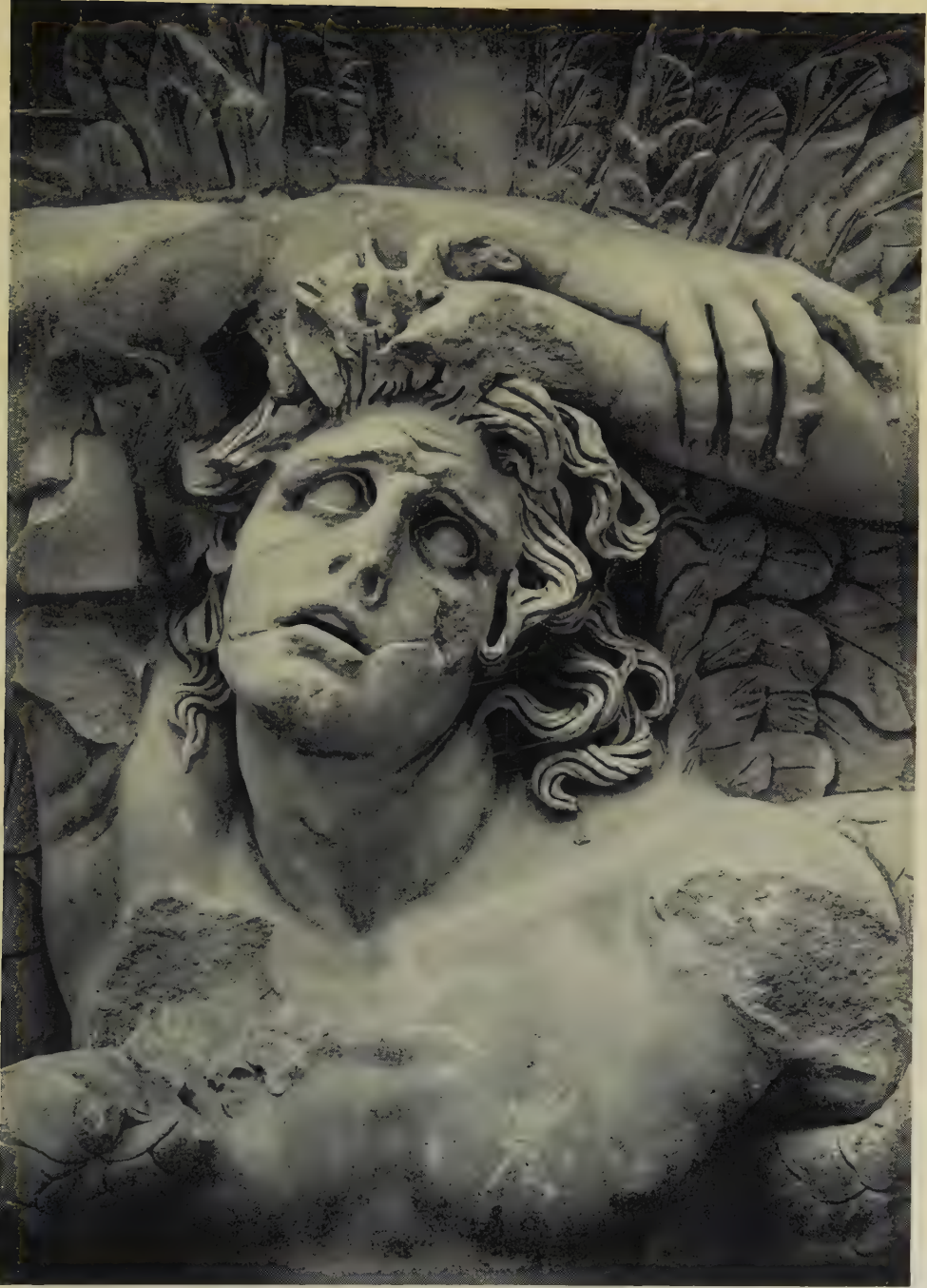
c. *Berliner Illustrirte Zeitung*, 17 August 1930.

d. *Pantheon* (Bruckmann, Munich) April 1931, pp. 167–171 ('Die Kunst des Tell Halaf', by A. Ungnad; well illustrated).

Since then Freiherr von Oppenheim's book has been published :—*Der Tell Halaf* (Brockhaus, Leipzig, 1931). It is not the full scientific publication, but an admirable general account which will be appreciated by both the general reader and specialists. We hope to publish a review of it at an early date.



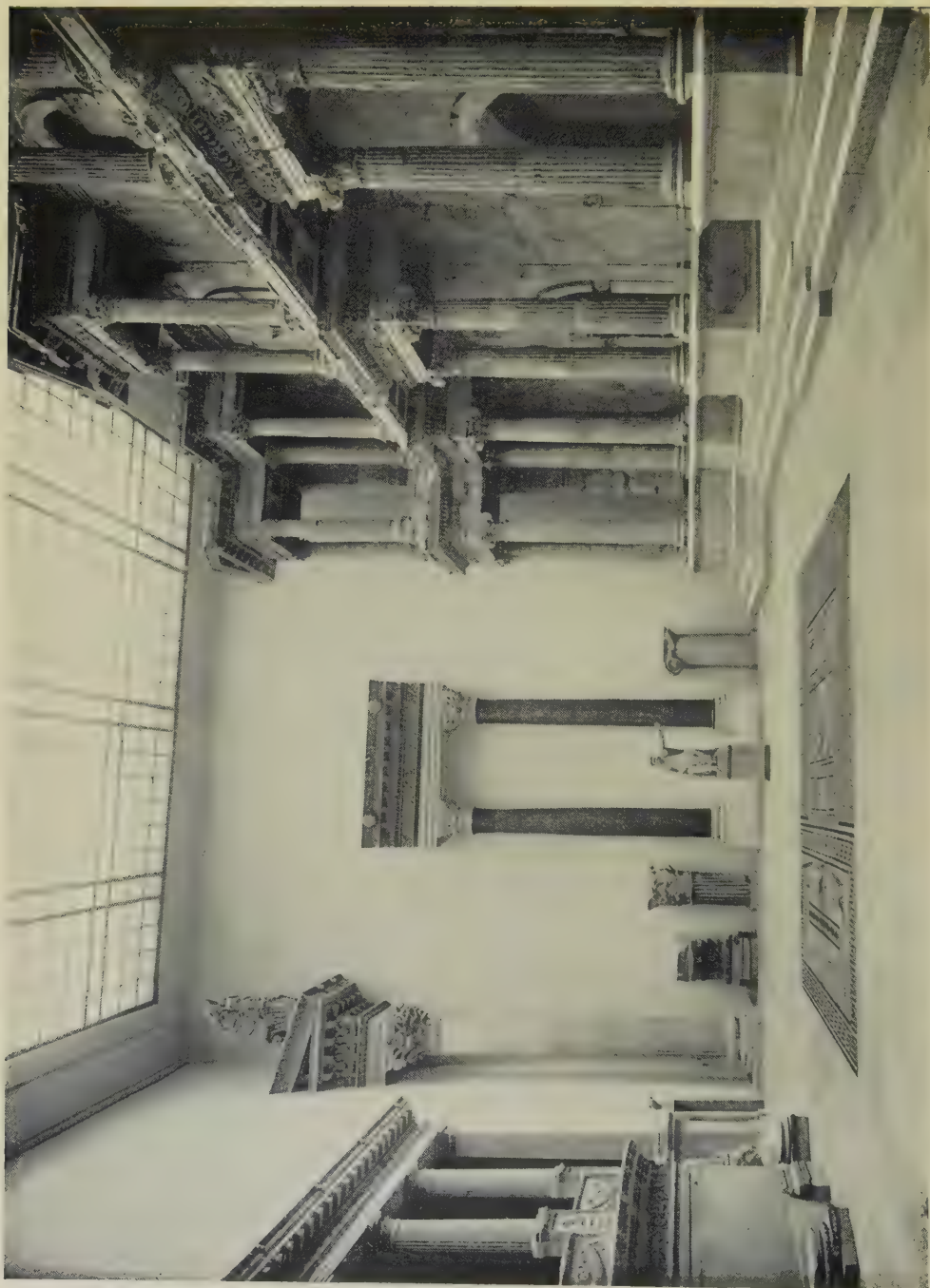
PLATE II



DETAIL OF SCULPTURE FROM THE PERGAMON ALTAR, BERLIN: ALRYONEUS

*Ph.* Pergamon Museum, Berlin

*facing p. 64*



TWO COLUMNS FROM BAALBEK  
*Ph.* Pergamon Museum, Berlin

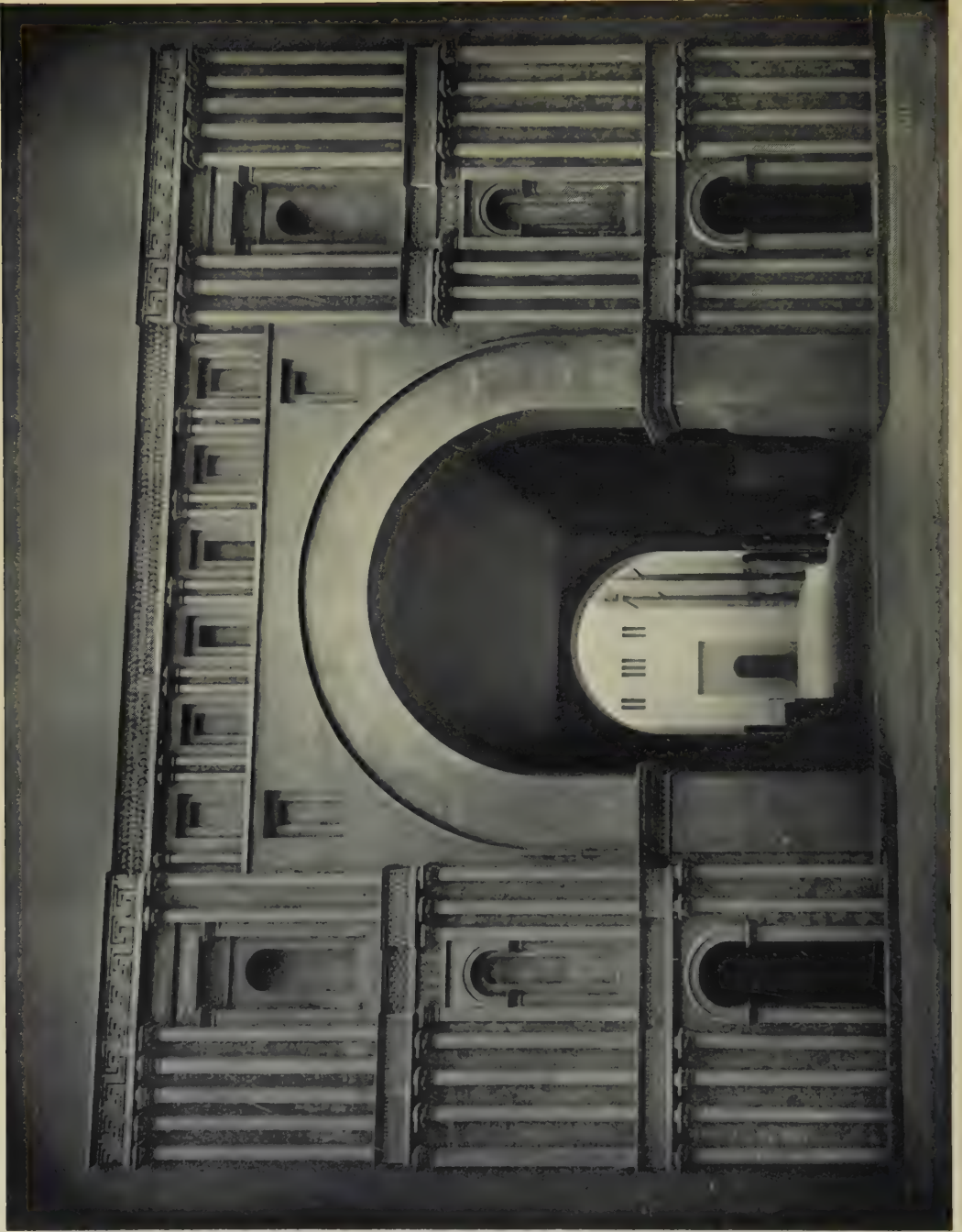
PLATE IV



PART OF THE ROMAN ROOM OF THE PERGAMON MUSEUM  
*Ph. Pergamon Museum, Berlin*



PLATE V



FACADE OF THE PARTHIAN PALACE AT ASSUR (c. A.D. 200), AS RESTORED IN VORDERASIATISCHES MUSEUM, BERLIN

PLATE VI



THE ISHTAR GATE AT BABYLON, AS RESTORED IN THE VORDERASIATISCHE MUSEUM, BERLIN  
*Ph. Vorderasiatisches Museum, Berlin*

PLATE VII

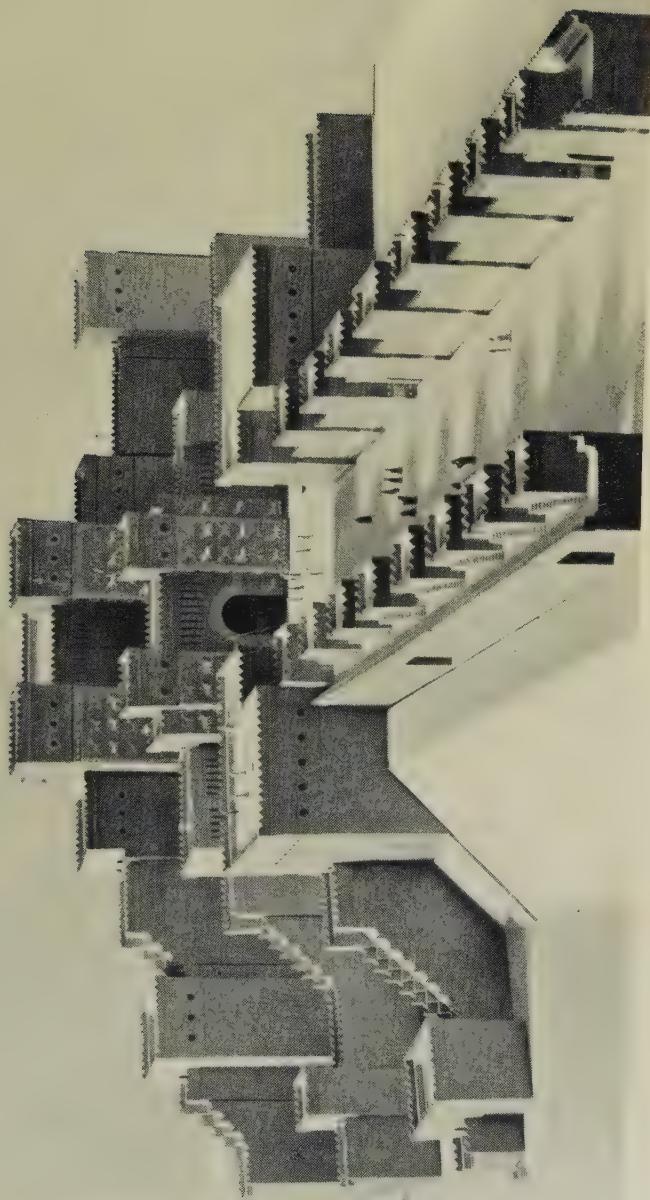


THE PROCESSION-WAY LEADING TO THE ISHTAR GATE AT BABYLON, AS RESTORED  
IN THE VORDERASIATISCHE MUSEUM, BERLIN

*Ph. Vorderasiatische Museum, Berlin*



PLATE VIII



MODEL OF THE ISHTAR GATE AND PROCESSION-WAY AT BABYLON, IN THE VORDERASIATISCHE ABTEILUNG, BERLIN  
*Ph. Vorderasiatische Museum, Berlin*



## MUSEUMS OLD AND NEW

Naval Expedition (from Sargon's Palace) loses most of its value because it is placed facing the light instead of at right-angles to it. This is made the more obvious by the adjacent stele of Naram Sin which, being placed at right-angles, is well seen in all its details. The whole of this (Asiatic) room is badly overcrowded. The Assyrian bulls look merely ridiculous. Lastly the labelling throughout, as in so many French museums, is deficient and even when present, unhelpful. The crabbed style of writing on the labels (so common all over Europe) should be definitely abolished.

In the Grande Salle de Suse (Salle VII), devoted to 'antiquités susiennes et perses' a brave attempt is made to reconstitute two little pylons from the Palace of Artaxerxes Mnemon at Susa, and two friezes of archers from the throne-room of Darius I. Both are artistically pleasing, though their restoration is not altogether satisfactory. They are set against a wall on which some tiresome and quite unnecessary modern frescoes in the Assyrian style distract one's attention. The room is about 100 by 50 feet in size, and about 20 feet high to the cornice. The lighting is bad; there are no plans, models or sections; and the labelling is inadequate.

For all its profusion of art inartistically displayed the Asiatic section of the Louvre is provincial compared with the majestic halls of the Pergamon Museum. Indeed provincialism is the curse of the Louvre, as of so much French work. The heavy hand of the petty bourgeois tradesman is felt everywhere. As one saunters through the stuffy crowded little rooms it is hard to realize that this is the famous Louvre; one keeps wondering when one is coming to the real thing—just as a stranger in a country town might search in vain for the main street where all the big shops are. The illusion of an antique shop is maintained to the end when, at the entrance, one finds a vast hall entirely devoted to the sale of pictures, casts, literature and even of human guides. One is irresistibly reminded of 'all them that sold and bought in the Temple' and their fate. 'Nothing for nothing and damned little for sixpence' is a mean motto for a national museum.

The overcrowding and general unsuitability of the show-rooms originates in the attempt to use a building for a purpose for which it was never intended. Dark alcoves may have been in place in a royal palace, but in the 20th century we have no more use for such things than for the petty intrigues associated with them.

Yet another criticism occurs. The Louvre occupies a huge area



## ANTIQUITY

in the heart of Paris, an area of fabulous monetary value.<sup>3</sup> But it does not even occupy the whole of it. To say nothing of the larger western court (Cour du Carrousel), there is a huge inner court (Cour Carrée), covering about an acre and a half, of no architectural or artistic merit and utterly useless. No attempt is made to render it either useful or beautiful.

Destructive criticism—and we repeat our criticism is made wholly in the abstract, without the slightest personal reference—should be followed by constructive suggestion. Having destroyed the Louvre what are we going to build on its site? We shall go to those experts in modern construction who have to meet the demands of modern enterprise—demands for business premises, cinemas and such like. There are already several examples, in Paris, Berlin, Vienna and elsewhere (even in poor old London), of buildings which are both aesthetically pleasing and functionally efficient—the two are inseparable. They are heralds of the dawn of applied science. They are not built in any ‘traditional style’ because they are themselves the first experimental ventures in a new and living style. Some are perhaps a little crude, because all first attempts are crude. But they are at least unaffectedly honest, unpretentious and alive, and they seem to satisfy, for though expensive they are multiplying rapidly. They represent the application of science to building; and they have their counterpart in those ultra-modern but equally pleasing and equally experimental cottages that were to be seen at the housing exhibition at Berlin last summer. These new ventures in architecture foreshadow a new era, and symbolize the spirit of an age which has cast off authority and outworn traditions. This spirit is rapidly becoming self-conscious, and is expressing itself unmistakably in science and politics. It is still immature and it may of course be submerged or stifled before it reaches maturity. Against it are arrayed the combined forces of tradition, vested interests and inertia. But, in museum-craft (which is the subject we are now concerned with) the ideal has been partially achieved once in the face of these and other even more formidable difficulties. In field-work and excavation science has already triumphed, and now at last it has begun to invade the dusty precincts of museums and workshops. The results will be far-reaching and impressive.

But as yet only a beginning has been made. At Berlin certain

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<sup>3</sup> The Louvre itself, without its contents, has recently been valued at £400,000,000, according to a report quoted in the *London Evening Standard*, 27 November 1931.

## MUSEUMS OLD AND NEW

periods of the past have been isolated and illustrated by means of their architectural remains. This has been admirably done, but there is a vast mass of other material which may be similarly treated. What principles are to guide the selection and display of statuary, paintings, embroideries, and all the mass of 'art mobilier' (such as all that on the upper floor at Bloomsbury)? In a great metropolitan museum, with treasures ransacked from all the ancient civilizations, the only rational scheme to adopt is one which shows the rise and decline of each successive cycle of culture by means of chronological arrangement. Selection will be necessary, and there will have to be drawers, and eventually perhaps whole galleries, and even buildings, for specialists. But this will be a help rather than a hindrance, for the evolution of culture can best be shown by exercising strict economy of exhibits. The surplus, if any, can be distributed to local museums, under adequate guarantees. But this is not the place for the detailed discussion of such a vast problem; one can only indicate certain general principles. Those who wish to read more about this matter may be referred to what I wrote about it more than ten years ago.<sup>4</sup> On reading it over again for the first time since it was written I see nothing to alter, and little that should be added except in amplification of the ideas there set down.

At this point in my original essay I proceeded to consider the British Museum. It seemed incumbent upon me to do so after criticizing the museums of other countries, however unwilling one might be to perform a thankless task. Since then however there have been rumours of impending changes, and it seems best to say nothing. One would in any case be preaching to the converted. There remain of course other openings for criticism; just as there are many things (such as the laboratory work, special exhibitions, free lectures and labelling) that one would have selected for praise. But the good points are obviously to the credit of the existing staff and their immediate predecessors, and the bad ones just as obviously a legacy of long ago. Consequently one felt even more strongly than elsewhere that, given the means and opportunity, such problems as lighting, arrangement and display would be successfully solved.

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<sup>4</sup> *Man and his Past* (Oxford, 1921) chapter 18. Some severe but justified criticisms of museums will be found in Sir Charles Close's Presidential Address to the Hampshire Field Club on 'The Deadliness of Museums'. It was printed in full in the *Hampshire Chronicle* for 2 May 1931 and will appear in the *Proceedings* of the Club.

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There is one matter however that cannot be passed over in silence. From a most useful handbook<sup>5</sup> one learns that, in common with all other British national museums and galleries, the British Museum is closed on Sunday mornings. Now Sunday is a day of rest and recreation; it is the only such day in the week for many hard-working people who have a right, as taxpayers, to demand consideration. One knows the real reason of course; but in this, as in other public affairs, there should be a policy of the open door rather than of the protection of vested interests. The practical difficulties of all-day Sunday opening have been overcome abroad, and a change is overdue.

Destructive criticisms of museums as they are in the world of today is easy. Constructive suggestions about museums as they might be in the world of one's dreams are equally easy, but unprofitable. What is required is a concrete adjustment of one's ideal to the world as it *will* be tomorrow, and this is impossible until we enter a more settled period. Certain developments, however, may be indicated as likely to take place during the present or following centuries.

In the first place it seems certain that the 'museum method' will be employed far more widely than at present, and will not be confined to pure science. There will be an immense increase of museums devoted to some special subject or area, or to special aspects of a group of subjects—in other words, of 'specialist' museums. This will leave the field clear for 'popular' museums dealing with the generalizations of science—with for example the evolution of life and of man and his works. There will still remain big metropolitan museums at one end of the scale and small local collections at the other; and the local museum will attempt (as at Newbury and Hanover) to combine width of outlook with the proper display of local culture. But the broad sweep of history and evolution will hardly be fully realized without a visit to one of the metropolitan museums.

Then again I think that museums will acquire souls. At present most of them are dead bodies. A live museum is one which is inspired by the spirit of science. A museum of palaeontology, for instance, should by means of its arrangement illustrate the evolution of life. It should be made absolutely impossible for the visitor to leave it

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<sup>5</sup> The National Museums and Galleries of London: lectures and special exhibitions for October 1931. H.M. Stationary Office, 4d. (Although nowhere stated, we presume this is a monthly publication).



## MUSEUMS OLD AND NEW

without learning something about evolution. It should be a teaching institution. The arrangement of exhibits, and of the accompanying explanatory distribution-maps and time-charts, will be regarded as of more importance than the acquisition of specimens. One cannot of course, in a museum, have arrangement without exhibits (though one can have casts); but exhibits without arrangement are a useless encumbrance, and serve rather to kill interest than to awaken it.

It is the old antagonism between the generalizer and the specialist. There is no earthly reason why one should not be both; indeed one is not really a man of science at all unless one can appreciate the full force of generalization as a stage in scientific progress. But instead the specialist often regards the generalizer with suspicion; and he in turn is apt to lump all specialists together as pedants. The one forgets that all generalizations are based upon special research, the other that outside one's own subject one is perforce dependent upon general ideas, general conclusions. In the individual and in the race knowledge advances by means of such generalizations; and the man in the street (who after all pays the specialist his salary) may reasonably demand to be taught the main outlines as well as the details. He can at present learn, if he submits a coin, a brooch or a potsherd, when each was made and where. It should be made easy for him to fit that object into any historical system by a visit to the show-cases. Suppose that the coin is Parthian, the brooch of the Peschiera type, and the potsherd La Tène III b; the historical meaning and content of these necessary terms should be visually displayed there—the culture of the people who made them. If he can thus find out in the showrooms, something about the Parthians—when they lived and where, and what sort of people they were—the coin may have started a train of interest that will fire his imagination and lead him on to investigate further. The interest of most people begins in some such way with something of their own they have actually found or seen themselves. Under favourable circumstances it often develops; but we have known of cases where it has died in a museum, though not always 'from natural causes'.

Such schemes involve building on a large scale. The Pergamon Museum points the way. There the preliminary problem was, one would imagine, straightforward; special buildings had to be designed for certain definite exhibits. The problem of designing a whole series of museums that shall be in alliance with science on the one hand and education on the other is far more difficult. Science we have, and of the best, but have we anything yet that can properly be called education?

## ANTIQUITY

So far as staff and exhibits go we have magnificent raw material which is not always used to the best advantage. We suffer, it is true, from certain disadvantages of climate and traditions, but they can and will be overcome. Meanwhile the formulation and discussion of such schemes for future development will make them easier to carry out when the time arrives. Successful revolutions are those whose future policies have been most carefully worked out beforehand.

### NOTE

It should be stated quite clearly that in the concluding paragraphs I am referring to museums in general, with no particular museum in mind. In many, a brave attempt is being made to break with the evil traditions of the last century.

# The Site of Troy

by WINIFRED LAMB

THE name of Troy, more touched with glamour than any other name in history, brings to the archaeologist, the literary critic and the ordinary man, associations so widely different that they are in danger of becoming altogether divorced. In the mind of the archaeologist, it calls up a series of unsolved problems that produce a faint sensation of irritation and anxiety. Can that tantalizing group of objects from Schliemann's excavations, most of which have been arranged more according to style than to stratigraphy, be regarded as a safe guide to the development of prehistoric Asia Minor? Which of the stone battle axes come from Town I and which from Town II? What really happened between the second and the sixth settlement? How does the material fit in with that from other early settlements in Asia Minor and adjacent lands?

The literary critic too is worried when he has to deal with the topography of the Troad in relation to the events described in the Homeric poems. Knowing much more about the latter than the former, he fails to fix a standard as to how far they should correspond.

The ordinary man, happily unconcerned with questions like these, remembers only with a certain thrill that the drama of the Trojan war found a fitting counterpart in Schliemann's dramatic discovery of seven—or was it nine?—superposed cities. He makes a mental note to ask the next archaeologist he meets how many there really were.

Much apparent coincidence is subconscious tendency, and we may explain as we please the fact that within the last few years both popular interest and scientific attention have been directed to Troy by a series of controversies, discoveries, and publications which have no obvious connexion with each other.

The excavation of a new settlement closely akin to the early stages of Troy in an area mentioned by Homer as the southern boundary of Priam's kingdom has solved some of the problems which used to handicap the archaeologist, while merely underlining others. A short



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account of this excavation, which was begun two years ago at Thermi in Lesbos, will be given below.<sup>1</sup>

Homeric scholars, meanwhile, have been disturbed by the revival of doubts long dormant as to the relation of Troy, Hissarlik and the camp of the Achaeans. Some six years ago, Dörpfeld, Brückner and others voiced the view that there was no room for the Achaean camp and the advances and retreats which the *Iliad* describes between Hissarlik and the Hellespont, and since then a series of treatises has been written to prove either that Hissarlik is not Troy or that the Greek camp was not on the Hellespont. The latest of these, by M. Vellay, has just been given to us under the title of *Les Nouveaux Aspects de la Question de Troie*.<sup>2</sup>

Meanwhile another class of experts, the philologists, have been disputing as to whether the TAROISA on a Hittite text can be identified as Troy, giving the Homeric capital the sanction of a genuine historical document. Unfortunately the element of uncertainty, which lowers the value of the material remains and distorts the geographical problem, has entered this field of enquiry as well.

Finally the appearance of Ludwig's *Schliemann*<sup>3</sup> at this juncture is particularly opportune, since the curious personality of the great excavator (whose archaeological note books show him to be distinctly more scientific than his biographer allows) is one of the tests of the authenticity of Hissarlik and the significance of the finds.

Here we will concern ourselves with two groups of problems only, the geographical, as presented by M. Vellay, and the archaeological as affected by the excavations at Thermi.

It is a healthy sign when well established hypotheses are attacked ; all to the good, therefore, that M. Vellay and his predecessors should undertake this offensive in the Troad. M. Vellay's position may be summarized as follows :—

(1) That if the Greek ships were drawn up on the Hellespont, Hissarlik cannot be Troy, because there is not enough room for the camp and the manoeuvres ; and because the present course of the Skamander is not suitable for the Homeric narrative.

(2) That it is impossible both on literary and on topographical

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<sup>1</sup> Conducted by W. Lamb under the auspices of the British School at Athens. See *J.H.S.*, XLIX, 223, 224 and L, 247-8 ; also *The Times*, 11 August 1931.

<sup>2</sup> Paris, Société d'Édition 'Les Belles Lettres', 1930, pp. 134.

<sup>3</sup> Emil Ludwig, *Schliemann of Troy* ; Putnam, 1931, reviewed on p. 108.

## THE SITE OF TROY

grounds for the ships to be drawn up anywhere else. This is directed towards a recent theory of Dr Dörpfeld and others that they may have been at Besika on the west coast.

(3) Therefore, that Troy must be sought elsewhere than at Hissarlik, and Hissarlik explained otherwise. M. Vellay, reviving the thesis of Herr Seyk, explains it as the burial ground of the Achaeans.

In discussing the relation of Hissarlik to the Hellespont, and calling our attention to the small distance between them, M. Vellay is dealing with a well-worn theme; his remote predecessors are Strabo and Demetrios of Skepsis, his immediate forerunners Dörpfeld, Brückner, Seyk and others.

These are divided into two parties, one of which denies that the camp was on the Hellespont, the other that Troy was at Hissarlik. As most of the arguments advanced have been answered—or anticipated—by Leaf in *Strabo and the Troad*, I cannot do better than quote Leaf's statement that, as long as we do not accept the excessive estimate of Agamemnon's army in the *Catalogue*, 'an adequate force for the siege, or rather the masking, of a fortress at Hissarlik could easily find place along the sea shore from the lagoon to the In-tepe with a depth of say 400 to 500 yards; its outer lines of defence would be a clear two miles from the town; and the way in which the battle is described as surging backwards and forwards over the whole of this space in a single day shows that the poet cannot have had a much greater distance in mind' (p. 182). To this I may add that should the course of the Skamander not tally with the Homeric evidence, a point which seems to me 'not proven', there are two more easterly courses wherein it may have had its bed. It is certainly odd that M. Vellay ignores Leaf's arguments so completely, and only refers to *Strabo and the Troad* in one footnote, when he is otherwise so well informed on the literature dealing with his problem.

With Dr Dörpfeld's latest theory, that the Greek camp should be placed at Besika on the west coast, M. Vellay deals effectively. This theory involves a wider interpretation of the term *Hellespont* than that commonly accepted, for, according to Homer, the Greek camp was on its shores. Dörpfeld and his party are, on their side, able to quote authorities for extending the Hellespont as far as Besika and Tenedos: while M. Vellay, on his, demonstrates that Homer himself appears to confine it to the Dardanelles. He also shows very clearly that a camp on the west does not tally with a number of situations described in the

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*Iliad*, even though it be a suitable distance from Troy and on the appropriate side of the Skamander.

This aspect of the controversy is unimportant compared with the archaeological tragedy that lies behind it. In the year 1924 Dörpfeld,

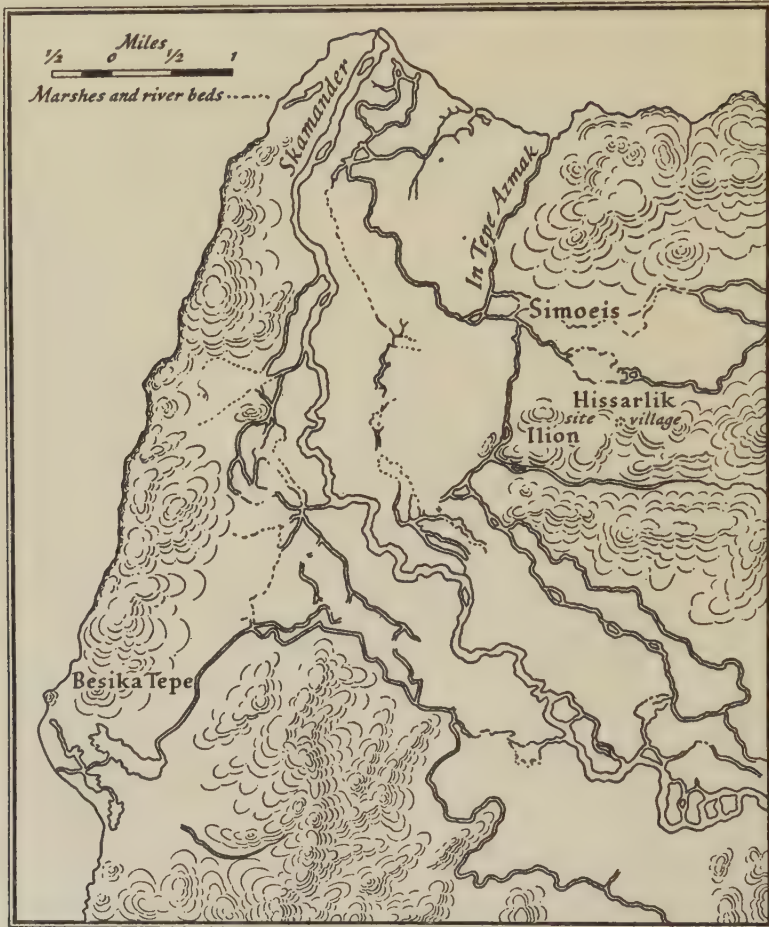


FIG. 1. The Troad (after Vellay)

Mey and Shede had the good fortune to obtain from the Turkish Government permission to conduct excavations near Besika. Here, with invaluable material for the prehistory of the Troad before them, two of the three scholars confined their attention to investigating the Homeric problem. To the third, Dr Mey, we are indebted for the



## THE SITE OF TROY

only publication of the dig which has yet appeared, for a very careful account of the geology of the area combined with an excellent section of the trial trench, and for a commendably honest confession that the opportunities for studying the pottery were neglected. Should such an opportunity again present itself, attention should be directed to a type of pottery decorated with linear patterns in varnish paint, unparalleled so far in Asia Minor but offering tantalizing possibilities of contact elsewhere. It was one of several wares dug up on the mound by Schliemann; though the stratification is unknown, style, at least, suggests that our patterned sherds belong to the early Bronze Age or even to the Neolithic period, while a distinct Troy VI element indicates that Besika was inhabited in the Homeric period as well.<sup>4</sup>

Anyone who has read *La Question de Troie* thus far cannot fail to be impressed with the author's competence as a scholar, and with his logical and impressive arrangement of material. These qualities carry with them, inevitably perhaps, a tendency to press the Homeric evidence too far and to demand geographical accuracy from a poet whose very identity is obscure. When one considers that the *Iliad* has its roots in the folk-memory of the dark ages, and was frequently trimmed and pruned before it grew to its final form, we must expect a certain picturesque exaggeration of the attacking and defending forces and allow for a landscape that does not in every detail conform to that of the Troad. In short, M. Vellay looks at his problem too much from one point of view, and this becomes all too apparent when he passes on to advocate Herr Seyk's preposterous theory that Hissarlik is the burial ground of the Achaeans.

It is almost incredible that such a theory should have been put before a generation as familiar with the theories and probabilities of archaeology as the present. The objections are so obvious that one would not enumerate them here were it not for the still more incredible fact that more than one scholar has shown an inclination to become converted.

In the first place, then, the burnt matter in Hissarlik VI is closely associated with buildings and is at the same time less extensive than in the second settlement; nor could it be mistaken for the remains of a cremation burial save by people totally ignorant of what cremation burials look like. Contrast it with the pyres at Halos, and beware of

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<sup>4</sup> Publication of the material will appear in the *Prähistorische Zeitschrift* for 1933.

saying, as M. Vellay does, that it has a close resemblance to certain Asiatic burial places (p. 112). No necropolis of the Troadic culture in Asia Minor has been dug except Yortan, where the bones of the dead are disposed in rows of pithoi; I am afraid he must be thinking of Bos Euyuk, which the first excavator believed to be the result of cremation, and which is now recognized as a village settlement frequently destroyed by fire. With considerable perspicacity, M. Vellay himself points out that one looks in vain for any likeness between his supposed tumulus of the Achaeans and anything discovered in mainland Greece.

In the second place, it would be very unusual, unparalleled indeed in the Aegean area, to have a cremation sandwiched between one series of settlements (nos. I-V) and another (nos. VII-IX).

The crowning objection, however, to the theory as expounded by M. Vellay is furnished by the diagram on p. 107. On the opposite page we are told that 'the fortifications of Hissarlik correspond exactly to the description given by Homer of the common burial ground'. (What quick work on the part of the Achaean troops!) The diagram shows an ideal construction drawn according to Homer's text, compared with the actual ruins of Hissarlik. We are certainly struck with the resemblance, till we realize that *it is the result of combining the walls of the sixth settlement with those of the second*. Moreover, is it not odd that a writer who accepts so easily the complete absence of his own Achaean fortification—('le mur qui traversait la plaine de l'est à l'ouest a naturellement disparu')—should object so strongly to the destruction of the north wall of Dörpfeld's Homeric city?

Hissarlik, then, is definitely a town site, and one of considerable importance. So much archaeology demonstrates, and any attempt to find another Troy must, as M. Vellay realizes, take it into consideration.

But what if there should, after all, be some other claimant lurking beneath an undug mound of the Troad? That is not impossible. It is sixty years since Schliemann's first excavations drew public attention to the ruins of Ilium, and nearly forty since Dörpfeld's brilliant analyses of architecture and strata concluded the campaign, yet the interval has added little to our knowledge of a district of Asia Minor, which, from every point of view, is of paramount importance. The most we can do, with the evidence at our disposal, is to try and estimate the position of Hissarlik by comparing it with other settlements belonging to the same culture; and even this comparison is more useful as a review of Western Anatolian archaeology than as a guide to Troy.

## THE SITE OF TROY

In the second half of the third millennium, the culture which we may call Troadic spread over a large part of Asia Minor: its extent has been discussed by Myres<sup>5</sup> and by Frankfort,<sup>6</sup> who illustrates it by an excellent map. The places marked on this map are, for the most part, those which have produced isolated or unstratified specimens of the wares of 'Troy' I and II; a few are sites which have been partially excavated, or explored in relation to alien and later cultures.

In short, when we try to detach and examine settlement sites which may be compared with Hissarlik, we can find only four where anything is known of the strata: Bos Euyuk in Phrygia, the Toumba of Protesilaos on the Dardanelles, Hanai Tepe in the Troad, and Senirdje in the south. In all these our information depends, at most, on trial trenches and clearings, an unsatisfactory method of excavation; at least, on measurements taken by archaeologists in cuttings made for commercial purposes. All have two features in common: they show strata destroyed by fire, and they tell us nothing, or practically nothing, about houses and fortifications.

During the last three years, a new factor has appeared in the prehistory of the Troadic people. Lesbos, less than an hour's journey by sailing boat from the Anatolian shore, was colonized apparently no later than Hissarlik and about the same time as the Toumba of Protesilaos. At Thermi on the east coast the settlers built a small harbour town, which their descendants destroyed, remodelled and reconstructed till the remains of five towns lay one above the other.

One by one these towns have been uncovered, and the excavation, now approaching its last season, of their remains has been comprehensive enough to give us the whole plan of the uppermost town (no. v) and of about three-quarters of each preceding settlement, so that, for the first time, architectural material can be placed beside that of Troy.

But whereas in Troy I all the buildings we have are incomplete—the ends of walls uncovered by Schliemann's north-south trench—Thermi I and II, both contemporary with Troy I, produced a large number of houses. These are for the most part long and narrow, with a large room at the back entered through one or more small ante-chambers. Certain rooms seem to have been devoted to the storing

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<sup>5</sup> Myres, *J.R.A.I.*, 1903, p. 369 ff.

<sup>6</sup> Frankfort, *Studies in Ancient Pottery*, II, 57 ff.



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of produce, for rows of pithos holes, some empty, some still containing fragments of store jars, run parallel with the walls. Hearths, common

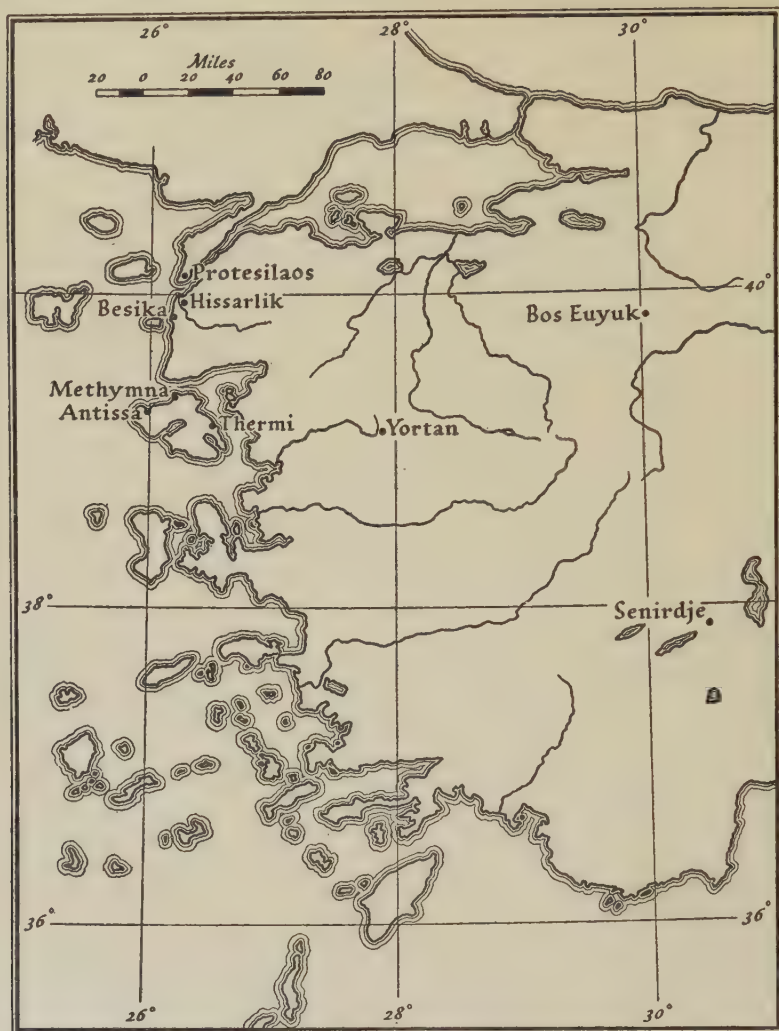


FIG. 2. Western Anatolia, showing settlement-sites

at all periods, may occur in any part of the room. They are carefully constructed in layers: first comes a stone foundation, next a layer of pebbles, then a mosaic of potsherds, and finally a burnt earth surface.

The typical pottery of the period consists of soft black polished

## THE SITE OF TROY

ware, and well baked red ware, both, of course, hand-made. Numerous small objects—copper pins, polished stone axes, bone tools—help to date the unstratified material from Troy I and II.

There follows a period when Hissarlik was deserted and debris accumulated on the site. To this interlude belong the third town of Thermi, the second village on the Toumba of Protesilaos, the cemetery of Yortan in Phrygia, and some of the pottery from Senirdje. At Thermi a new development occurs which throws light on the relation of Lesbos to mainland Greece. This is the introduction of *bothroi*, clay-lined pits below the floor or ground level, concerning the function of which there has been considerable controversy. Now these pits have long been known at Orchomenos, and have since been discovered in an early Helladic context at Gonia, Korakou, Zygouries and Eutresis on the one hand ; in an early Macedonian stratum at Molyvopyrgo in Chalcidice on the other. Since both Greek and Macedonian examples are dated by the pottery with which they are associated to be later than Thermi III, we cannot help suspecting that the practice of making *bothroi* must have travelled from east to west.

Meanwhile the pottery from Thermi and the contemporary Anatolian sites develops steadily, showing a marked improvement in technique, new forms and more varied decoration. The whole period, in fact, was one of growth and activity in which Hissarlik did not participate. Can it have attracted the attention of some enemy through being already more powerful than its neighbours ?

A more dramatic chapter in the prehistory of Anatolia opens with the founding of the second city of Hissarlik on a scale unparalleled so far in any contemporary town-site in the Aegean area. The magnificent fortifications, three times remodelled, and the stately approach to the centre of the town, are appropriate only to the metropolis of the Troadic culture ; nor could any ordinary town have acquired the magnificent gold ornaments, the superb lapis and greenstone battle axes, and the other treasures that give an impression of commercial enterprise and wealth which find their only counterpart in Crete. It is, indeed, difficult to imagine that two such cities could exist simultaneously in one district.

We may assume, therefore, that from Hissarlik radiated the activity that carried the Troadic culture into Macedonia and influenced so unmistakably mainland Greece and the islands in the early Helladic period.

Thermi, as we should expect, reflected the prosperity of the Anatolian capital. The fourth town was built over the ruins of the

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third, on entirely new lines, oriented northeast and southwest instead of north and south. But the final achievement of this little community was Thermi v, contemporary, as far as we can judge, with the middle phase of Hissarlik II. Thermi v is characterized architecturally by its wider streets and by the appearance, in its very centre, of a single house recalling at first sight the well known *megaron* type with antae and three inner chambers. It fails, however, to take its place at the head of the series of *megara* because it is not detached from its neighbours and because its hearth is not central. We must, therefore, abstain from connecting it with the slightly later megaron-house of Troy II 3, which is the forerunner of the palaces of Tiryns and Mycenae.

The wall which protected Thermi v was broken by two gateways; whether towns I-IV were also fortified we should learn during this year's excavation. The objects found within the various houses—vases, figurines, copper, stone and bone tools—link Thermi to the penultimate stage of Hissarlik II.

Now the final stage of Hissarlik II, characterized by the appearance of wheel-made pottery and by the megaron-house mentioned above, perished by fire at the beginning of the second millennium, while the villages of Bos Euyuk and Protesilaos seem to have shared its fate. The aggressors may, as Prof. Myres suggests, have been the Hittites. As for the Thermiotes, whose home shows no trace of violent conflagration, they may have anticipated the danger and migrated to another part of their island.

In any case, the Troadic peoples remained in a subordinate and devitalized condition through the confusing stages represented by the third to fifth settlements of Hissarlik; nor can we again pick up the thread of our story till the foundation, at a date still under dispute, of that city which so many scholars believe to be Homeric Troy.

So we find ourselves back at the sixth city of Hissarlik where we originally started. Its walls, though different in style to those of the mainland palaces, are no less impressive; its surviving buildings, though few, are distinctive, and no one who has read *Troja und Ilion*, pp. 150-175 could say 'de cette Troie VI il ne reste rien, sinon un fragment de muraille'. Its area, which seems to M. Vellay inadequate, is practically the same as that of Gonia and not much smaller than that of Phylakopi: it seems to conform to the standard of the times, though I must confess that there is very little material in the Aegean available for comparison owing to the scarcity of town-excavations.

The finds from Hissarlik VI deserve special mention, for they



PLATE I



THERMI : PART OF TOWN V

PLATE II



THERMI: VASES FROM TOWNS IV AND V

## THE SITE OF TROY

include, besides Mycenaean sherds, a peculiar type of grey pottery, sometimes plain, sometimes decorated with wavy lines or with attachments in the form of horns and animals' heads. This pottery is of all Troadic wares the most interesting: on the one hand, its connexion with Minyan ware, first demonstrated by Mr Forsdyke,<sup>7</sup> is becoming more apparent each year; on the other, the possibility of its development into the Aeolic bucchero of the seventh and sixth centuries is indicated by recent finds from Methymna and Antissa in Lesbos. Its position in the history of ancient pottery would, I think, become quite clear if we had stratified material from the rest of Troad to put beside that from Hissarlik; unfortunately so little study has been made of Troadic archaeology at this period that we can predict nothing with certainty. Compare for a moment our knowledge, slight as it is, of the early Bronze Age in these lands with our almost complete ignorance of the period between 2000 and 1100 B.C. Hissarlik stands isolated in the general obscurity, and if it seems singularly defenceless before the attack of M. Vellay, it is after all the archaeologists who are to blame.

For, if there is any moral to be drawn from this mixed account of M. Vellay's book, the prehistory of northwestern Asia Minor, and recent discoveries in Lesbos, it is the need for further excavation. Thus only can we get a reliable solution of the problems of archaeology, ethnology and topography which form such a handicap to research. A comprehensive examination of the early remains in the Troad will show once for all if any parallel exists to the hitherto unique ruins excavated by Dörpfeld and Schliemann, and prove their right to bear the name of Troy. To Lesbos as well as to the Troad we may look for a sequence of wares covering the Hissarlik III-V periods, and for information about the development of the later grey wares; but traces of invasions, such as those which Prof. Myres postulates from the dolichocephalic skulls in the Third Settlement, and of oriented contacts, like that between Troy and the Hittites, must be sought in Asia Minor itself.

There is reason to hope that this will become possible sooner than any of us anticipated, and that the clue to what we seek will be placed in reliable hands. In the meantime, every sign of interest on our part, every small concession won, is a step in the right direction and increases the likelihood of ultimate achievement.

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<sup>7</sup> *J.H.S.*, xxxiv, p. 144.



## Notes and News

### NENNIUS'S CHRONOLOGICAL CHAPTER

Mr A. O. ANDERSON has sent us the following extract from an unpublished work of his on 'Materials for the History of North Britain in the Fifth Century':—

HISTORIA BRITTONUM, chapter 66 (translated from the text in MONUMENTA GERMANIAE HISTORICA, AUCTORES ANTIQUISSIMI, XIII, 209).

From the world's beginning to Constantinus and Rufus [A.D. 457], 5658 years are found (1).

Also from the two Gemini, Rufus and Rubelius [29], to the consulate of Stilicio [400], are 373 years (2).

Also from Stilicio to Valentinianus, son of Placida, and the kingdom of Guorthigirnus, 28 years (3).

And from the kingdom of Guorthigirnus to the quarrel of Guitolinus and Ambrosius are 12 years ; which is *Guoloppum*, that is, *Catguoloph* (4). And Guorthigirnus held empire in Britain when Theodosius and Valentinianus were consuls (5); and in the fourth year of his reign the Saxons came to Britain, when Felix and Taurus were consuls [428], in the 400th year from the Incarnation of our Lord, Jesus Christ (6).

From the year in which the Saxons came to Britain and were received by Guorthigirnus, down to Decius and Valerianus, are 69 years (7).

#### NOTES

1. *reperiuntur* ; in Victorius, *referuntur*. 5658 A.M. = A.D. 457. This calculation is taken from some copy of Victorius of Aquitaine's *Cursus Paschalis* (M.G.H., *Auctores*, IX, 682), which was published to take effect from A.D. 457.

2. This means that Stilicho was consul in 373 A.P. (Victorius) = A.D. 400, and is correct. The consulate of Rufus and Rubelius (in reality A.D. 29 ; but a year earlier, according to Victorius) was the year accepted by Victorius as the year of Christ's crucifixion. The calculation in the text was taken through Victorius from Prosper, and

## NOTES AND NEWS

also from Sulpicius Severus's History, II, 27 (*Patrologia Latina*, book xx, 144).

3. From the consulate of Stilicho (A.D. 400) to the reign of Valentinianus and Placidia (A.D. 425) are 25, not 28, years. This is a calculation, made in connexion with the *Historia Brittonum* extracts from a *Liber S. Germani*, of the date of Germanus's visit to Britain; it is no doubt derived from Prosper, who places the visit in A.D. 429. As it stands, the calculation in the text would make Valentinianus and Vortigern begin to reign in 428; but that is probably intended for the year of the visit of Germanus; especially if the date given below of Vortigern's accession (425) was entered by the same writer. The Lives of Germanus say that Germanus (†448), on his second visit, left Britain after the death of Vortigern (†446×), and died in the reign of Valentinianus III (†455) and Placidia (†450): but it is not there stated that Vortigern reigned in Britain at the time of Germanus's first visit.

4. This is derived from some Welsh source, now lost. The word *Guoloppum* (erroneously *Guoloppuni*, in Harleian MS. 3859, fo. 189 verso) probably means 'at the villages of Wallop', on the Hampshire river Wallop; *Catguoloph*, 'the battle of Wallop'. See O. G. S. Crawford, in *ANTIQUITY*, v, 236-8.\*

5. This statement that Vortigern began to reign in the consulate of Theodosius and Valentinianus (A.D. 425) is derived from the statement in chapter 29 of *Historia Brittonum* that the previous ruler in Britain, Maximus, was killed by those *consuls*; a statement erroneously derived from Prosper of Aquitaine, who says that Maximus was killed during the reign of the *emperors* Theodosius and Valentinianus (A.D. 379-391).

6. By 'the fourth year' of Vortigern, A.D. 428-9 is meant: Felix and Taurus were consuls in 428. For the 'Incarnation' we must certainly read 'Passion'; and Mr Anscombe says that most MSS. have '401st'. 401 A.P. (Victorius) = A.D. 428. But the writer of this chapter 66 seems to have placed both the arrival of the Saxons

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\*Since Mr Anderson's note was in print, I have discovered another Wallop in Shropshire. It is not unlikely that this may have been the original name of the river Rea, rising northeast of Montgomery and joining the Severn near Shrewsbury. Further research is required here to discover early forms and the precise attribution of the name.—O.G.S.C.

## ANTIQUITY

and the visit of Germanus in the one year, A.D. 428. This is a deduction from the *Liber S. Germani*, and has no independent authority.

7. 69 years after 428 would bring us to A.D. 497; Decius was consul in A.D. 486. There is therefore some textual error, which would no doubt be explained if we found the source from which this calculation is derived.

According to De Rossi, Paulinus, the consul of 498, was a Decius (*Inscriptiones Christianae urbis Romae*, I, 493; cf. Mommsen, *Auctores*, XII, 495, s.v. Inportunus). De Rossi based this assertion upon the Letters of Cassiodorus. The more probable explanation of the text is that there is an error in the numbers of the calculation. For 'to Decius and Valerianus' we must read 'to Decius', with Gutschmid and Mommsen; and for 69, perhaps 59 years.

Chapter 66 of the *Historia Brittonum* is a collection of quotations and deductions drawn from known and unknown sources. It has no original value for the date of Vortigern's reign, or for the date of the arrival of the Saxons.

Immediately after the passage translated above, and before Nennius's 'Marvels of Britain', the 'Annales Cambriae' and Welsh pedigrees are entered in Harleian ms. 3859, fos. 193-5 verso.

## RECENT DISCOVERIES IN SHETLAND

At the extreme southern end of Shetland, and on the western coastline of the low-lying isthmus that links the Ness or headland of Sumburgh to the mainland, there was discovered some 25 years ago a group of prehistoric ruins that centred round a broch, to which the bulk of them were secondary. A large part of the group was thoroughly explored at the time by the proprietor, the late Mr Bruce of Sumburgh, but progress was hampered by the existence of a ruined dwelling having no great antiquity, but over which Sir Walter Scott had cast a glamour, bestowing on it the fictitious name of Jarlshof. The various remains on the site having been handed over to the care of H.M. Office of Works, it was decided to make a further exploration. The supervision was entrusted to Mr A. O. Curle, F.S.A., and results of particular interest were obtained last summer in the limited period during which the excavation was carried on. At a distance of some 50 yards inland from the sea, and in rear of the secondary buildings above referred to, there was found a dwelling sunk as regards its floor level to a depth of 6 feet at shallowest below present surface, and some 5 feet below the ground







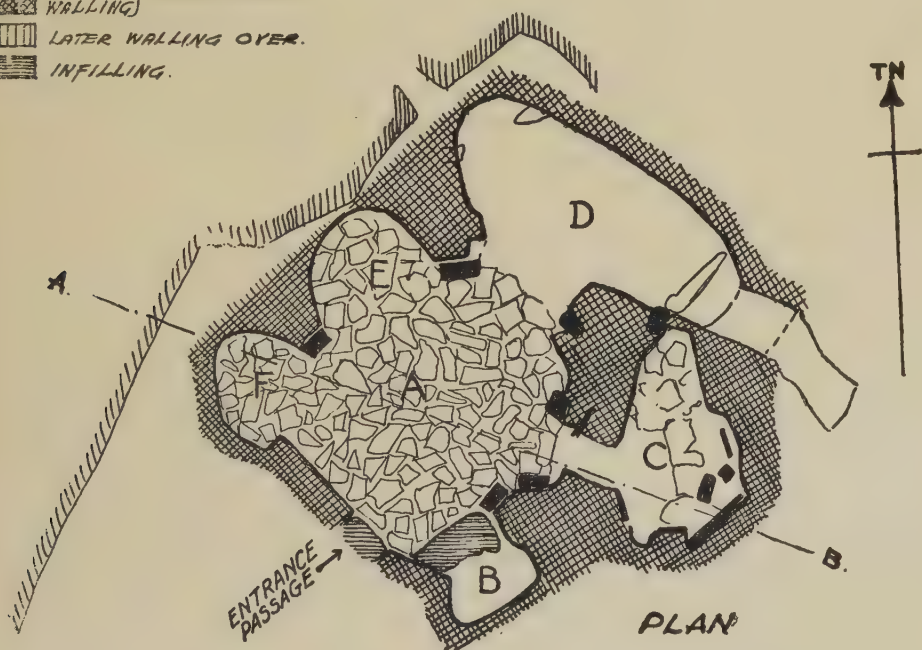
# NOTES AND NEWS

## JARLSHOF, SHETLAND CHAMBER



SECTION A.B.

-  UPRIGHT
  -  WALLING
  -  LATER WALLING OVER.
  -  INFILLING.
- } SAME PERIOD.



0 5 10 15 20 25 FT

AUG. 1901.

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level of its epoch, with walls formed of flat unhewn sea-worn boulders still standing to a height all round of from 3 to 4 feet. The exploration of the interior was complete up to the inner end of the entrance passage, and revealed a house measuring some 18 by 20 feet in length and breadth, consisting of a main chamber, or hall some 12 feet long, out of which there opened two large chambers and three cells of which the hall and two cells were carefully paved. One of the larger chambers had been used as a cook-house for in it were found the hearth, food refuse, bones of domestic animals, of birds and of fish, cereal grains, portions of saddle querns and rubbers. The bulk of the remains found disclosed a neolithic culture, rude stone axes, saws of slate, scrapers of white quartz, and adzes of bone; but the discovery of a number of fragments of clay moulds for casting bronze implements, among which pieces of moulds for a sword and a bronze socketed axe are recognizable, show that the period of the occupation is clearly of the Bronze Age. The significance of this discovery is marked by the fact that the record of bronze in Shetland is confined to one object, a spear-head. An interim account of the excavation has already been communicated to the Society of Antiquaries of Scotland, and it is hoped, if funds permit, that the work may be continued next summer.

### THE PLETTENBERG SKULL

Dr L. H. DUDLEY BUXTON writes:—

‘The recent discoveries of early man and of his artifacts in South Africa have excited considerable scientific interest, and recently we have had the opportunity of examining various skulls and a long series of beautiful photographs of existing racial types from that region in the Wellcome Historical Medical Museum in Wigmore street, London. Professor Drennan, who has done so much to further these studies, now reports a new skull from Plettenberg Bay, about 300 miles east of Cape Town. No data are at present available as to the age of this interesting specimen, and details of its exact character are lacking. It appears however to be remarkable for its extremely large size—a characteristic, it will be remembered, of the Boskop skull—and to be remarkably different from the Bushmen, who are notably small brained. Professor Drennan is of opinion that this large size is due to the retention of foetal characters in the adult, and that the skull represents a forerunner of the Bushman, and shows how man has arisen from a lower form. Without further details it is difficult to discuss critically Professor Drennan’s most interesting theory. The position of the Bushmen is a

problem of considerable difficulty. In some ways they are noticeably different from the other races of man ; presumably they originated in South Africa. Sir Arthur Keith has drawn attention to the affinities of the Boskop type—to which as far as one can gather from the scanty details this new skull belonged—with the Bushman-Hottentot type, but differing from them in having large brains. It is not improbable that we are actually dealing with a type that is akin to the ancestral type of man, although already specialized in the Bushman direction. The actual mechanism of evolution must be considered uncertain, and therefore the publication of Professor Drennan's detailed studies will be awaited with particular interest'.

### THE ANTIQUITY OF MAN

MISS CAROLINE RYLEY writes :—It has always seemed to me that arguments for the antiquity of Man have been based too exclusively upon 'finds', and too little upon other, possibly equally cogent, considerations. The conclusion has been forced upon me that the dating based upon finds cannot possibly explain the equally well-established facts of distribution.

The first, and fundamental, hypothesis that I would like to put forward is, that most of the 'finds' of sub-Man himself are not, as has been implicitly assumed, *representative* of the period to which they belong, but *survivals* from an earlier time. Almost a corollary to this is the hypothesis that the locality in which they have been found was, so to speak, a 'back-water' region of their own time—this fact helping to explain the reason for their survival both in their lifetime, and, as fragmentary remains, to this day ; the more progressive regions having ousted them in their day, and swept away their remains in the more active life that followed. Colour is, incidentally, lent to these time-hypotheses by the 'marginal' character of the localities in which were discovered the remains of *Pithecanthropus* and *Sinanthropus* respectively. Such marginal regions might well allow an out-of-date form of life to survive while the rest of the world was (comparatively speaking) hustling forward ; and allow their remains to lie undisturbed until the changes due to lapse of time themselves would aid in the work of permanent preservation.

To regard *Pithecanthropus*, say, as a survival rather than as representative of his age would help to explain an otherwise inexplicable fact of the widespread distribution, during the Ice Ages, of a 'man'



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(whether sub-Man or *Homo sapiens*) intelligent enough for the Chellean and Acheulean cultures.

There would seem to be grounds of distribution and differentiation for the assumption that the work of the Chellean and Acheulean periods was that, not of sub-Man, but of *Homo sapiens* himself. . . . For *Homo sapiens*, at the close of the Ice Ages, is not only 'evolved', and in possession of a strictly limited field—he is widely distributed over areas which have been barricaded off from each other for the greater part, if not the whole, of the Ice Ages. Not only this, but he has been long enough in existence to have evolved physical types, or 'races', of which the fundamental characteristics are so deeply rooted as to be permanent—or at least to have survived until now. Upon what hypothesis can this world-wide distribution of *Homo sapiens* at the close of the Ice Ages—this deeply marked differentiation into races—be explained, if not by the assumption that, at the opening of the Ice Ages, *Homo sapiens* was not only already in existence, but already distributed over areas which, in the Ice Ages, became almost impassably separated from each other (thereby giving the conditions for differentiation) ?

As to *where* the emergence took place, and where was the centre of radiation—that is an entirely different matter. Judging from the facts of distribution, one would expect it to be fairly central as regards the 'Old World'—one whence a distribution and a growing differentiation could be carried on in Europe, Asia, and Africa, more or less 'pari passu'. Judging from established facts of later developments (especially those concerned with the emergence of civilization from a mere food-producing stage) one would expect the evolution from sub-Man to have taken place under conditions which were at the same time stimulating (or 'urgent') and encouraging—such conditions as may perhaps be roughly approximated to those of our present-day 'Warm Belt'.\* Possibly the Chellean and Acheulean artifacts are those of *Homo sapiens* in situ (though not delimiting his distribution); forced southwards and sunwards by recurring Ice Ages, but returning in the warm interludes of which the so-called 'Post Glacial' may be one. In this later 'Warm Stage' he would, however, find his more congenial habitat somewhat further south than formerly—*i.e.*, in the Mediterranean and similar regions. Though adapting himself also to harder conditions in a colder clime, here would, and did (?), take place, his

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\* The later developments have been worked out in detail, but quite independently, by De Geer ('The subtropical belt of Old World Empires', see pp. 118-20, *post.*)—ED.

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evolution of the food-producing stage of culture, and of that more properly called civilizations.

The conditions of the ' Warm Belt ' have always, as far as facts can be established, been conducive to ' progress ' properly so called. To it belong, probably, the emergence of man from the ' savage ' stage of a dependence upon wild ' nature '. To it belong, certainly, the first perfecting of beautiful handicrafts, the first shaping of efficient tools in metal, the first reliance on acknowledged law, the first architecture, art, literature; the first conception of spiritual religion. The great achievements of the Greeks and, later, of the Lombards, were those of a race with the physical and mental robustness of cooler climes plunged into the opportunities of a civilization evolved in warmer regions (now grown too warm for the maintenance of an effectively progressive physical type). The achievements of early modern ' Western ' Europe are those of the same Northern races enkindled by (mental) contact with the South, through the Renaissance.

Possibly those conditions which have produced the most notable established facts of human progress, were those also of the emergence of humanity itself—of *Homo sapiens*.

However this may be—and wherever this emergence may finally be located—there is surely some solid ground, on the known facts of distribution and of differentiation, for the following hypotheses as regards the *time* of the emergence:—that the discovered remains of sub-Man are those of forlorn survivors of an otherwise already extinct type of *hominides*; and that humanity proper, *Homo sapiens*, had not only emerged, but was fairly widely distributed in the Northern Hemisphere before the oncoming of the Ice Ages.

### NEOLITHIC POTTERY

It is only within living memory that British neolithic pottery was recognized, but our knowledge of it since then has increased rapidly. The following is a summary of a recent lecture by Professor Gordon Childe. It first appeared in *The Times* (11 Nov. 1931), and as here printed it has the Professor's approval as a correct summary.

Professor Childe re-stated the division of British ' Stone Age ' pottery into two quite distinct groups—Windmill Hill and Peterborough wares. Dealing with the Windmill Hill class, he said that his collaborator, Mr Stuart Piggott, would show that despite local and temporal variations it preserved a striking uniformity from Sussex to the Orkneys, from the North Sea to the Atlantic coasts. The leathery character of

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the vases revealed them as belonging to a great family, extending all across Northern Gaul from the Rhine to Brittany and the Channel Isles, comprised under the general name Western. This family might be divided on the Continent into three principal groups, localized respectively in Belgium and the Rhine Valley, in the Seine basin and in central France, and in Brittany.

As a whole the British pottery showed no special affinity to any one of these, so that it might rank as an independent group that split off from the Western stem before the specialization of the rest. Exact agreements, however, linked a small sub-group of the Windmill Hill ware localized in Scotland and Northern Ireland, with a similarly restricted group in Brittany. These were the sole valid evidences for the theory which derived the Windmill Hill culture from Armorica and brought it to our shores along with long barrows and dolmens. That theory must, however, be rejected on other grounds and the agreements noted above explained by a reaction on Brittany from Britain. Windmill Hill culture either came from northeast Gaul across the Straits in pre-Megalithic times or direct from the Iberian peninsula with the chamber tomb complex, in which case the analogous cultures in Gaul would be parallel but independent offshoots of the same stem.

The affinities of Peterborough ware (which flourished principally in Eastern Britain) lay notoriously on the Baltic. A series of slides illustrated astonishing correspondence in curious patterns made with twisted threads and cords between British vessels and others from Denmark, Sweden, Finland, and Russia. The rise of this style could be traced only in the Ukraine, whence it was transmitted again in pre-dolmen times to Denmark, and eventually to Britain. This country was, accordingly, the meeting place of two cultural currents even at the beginning of the New Stone Age.

### THE BEAKER INVASION OF BRITAIN

Miss MARGARET MITCHELL sends the following observations on the paper in our December number by Mr J. G. D. CLARK, whose reply is appended:—

Mr J. G. D. CLARK has adduced evidence from England to show that the beaker invasion of Britain was of a dual character—a fact already brought forward by Professor V. G. Childe in his *Bronze Age*. The evidence from Scotland leads to a similar conclusion. Distribution maps show that the penetration of the 'A+C' complex has been principally by seaboard or river valley routes from the North of



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England. Yet beaker settlement sites at Hedderwick, Gullane Bay, North Berwick and Tents Muir in Fife—all suitable landing places on the East coast south of the Firth of Tay—have yielded 'A+C' sherds which show that some at least of the invaders must have come by sea. This disagrees with Mr Clark's assertion that the northern beakers demonstrate a subsequent development rather than a point of arrival. On the other hand the distribution of the B beakers in Scotland is predominantly coastal and points to a sea-borne invasion probably independent of the 'A+C' penetration.

In treating of the different types of beaker represented in Britain, Mr Clark has omitted to mention one important form, designated by Abercromby BC. It is due to the crossing of a pure B tradition with the debased C variety. Now in Scotland BC and C beakers predominate on the eastern seaboard of Aberdeenshire. It would seem therefore as though the B complex arrived after the 'A+C' tradition was already established there and that the resultant intermingling produced the hybrid BC. This conclusion would bear out Mr Clark's suggestion that there was a chronological difference in the arrival of the two complexes.

On the West of Scotland the 'A+C' group is well represented and includes three very early examples from Closeburn (Dumfriesshire), Stoneycirk (Wigtownshire), and Kilmartin (Argyllshire). Their provenance points to a relationship with the northwest English beaker group rather than with the Northumberland area, and they must be accounted as early as, if not earlier than, the East Scottish examples south of the Firth of Forth. Pure B beakers are represented on the West coast by single finds as at Largs on the coast of Ayrshire, or in settlement sites such as the Island of Coll and Ardnamurchan Point. Dr Cyril Fox (*Arch. Camb.*, 1925) has made the tentative suggestion that some of the West of England and Welsh beakers may have come direct by sea, but later he abandons the idea in favour of an overland route from East Anglia for which there seems much less evidence. I would here reassert the theory of a sea-borne invasion from the southwest as providing the most rational explanation of the distribution of B beakers on the West Scottish seaboard, as well as for the more northerly 'A+C' group in Lewis, Uist and Skye.

REPLY by Mr CLARK:—

I must hasten to agree with your correspondent that the evidence from Scotland confirms that of England and Wales in demonstrating

## ANTIQUITY

the dual character of the beaker invasion. My only reason for omitting the Scottish evidence was that the dearth of objects associated with beaker burials and the restriction of human settlement to a fraction of the country, owing to the infertile and inhospitable nature of the greater part of its area, combined to render unsuitable the application of my method of research.

It is necessary, however, to correct a certain misapprehension, for I appear to have been misread into asserting the northern beakers in general to 'demonstrate a subsequent development rather than a point of arrival'. If my remarks are re-read it will be appreciated that I refer only to the 'A+C' group (ANTIQUITY, 1931, p. 418 top). I agree entirely that the 'B' beakers of Scotland may well be explained as direct arrivals from the Continent, but, returning to the 'A+C' group, I do not see anywhere in my paper any claim that they arrived exclusively, whether by land or sea, from northern England. At the same time I feel myself to be on safe ground, when I say that, for the most part, 'they represent a subsequent development'. If I have made this 'assertion', I fear that I am unrepentant and will continue to make it until faced with evidence of sufficient weight to render my position untenable. It may not be out of place here to point out that this was the view of Lord Abercromby himself, so that my 'assertion' may be said to dwindle into a meek statement of the orthodox view. It is incumbent on your correspondent to find the destructive arguments: I am content to rest upon the very solid evidence adduced by Abercromby. Many beakers have been found since the publication of his great work, but there is no reason to suppose that they would materially affect the conclusion to be drawn from the following condensation of his table:—

Beaker types			'A'	'AC'	'C'
South of Humber	..	..	97	0	2
Rest of England	..	..	11	37	76
Scotland	..	..	3	27	130

I submit that the only possible deduction to be drawn from this table is that type 'C' beaker is a development from type 'A' via 'AC', the geographically intermediate distribution of 'AC' being most significant.

Your correspondent seems to be worried over my omission of any specific reference to type 'BC'. As it was embraced in my remarks concerning hybrids and is furthermore numerically unimportant in the area with which my paper dealt, I saw no immediate necessity for

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doing so. Since it is mentioned, however, we may enlarge upon its significance. In bringing forward further very welcome evidence in support of my suggestion of a chronological difference between the 'B' and 'A+C' complexes, your correspondent argues for the view, in which I concur, that the 'B' complex is the earlier of the two, producing by admixture the 'BC' group of which we are speaking. It will occur to the reader that, in view of the fact that Abercromby records no fewer than 126 'BC' beakers and not a single 'AB' hybrid from Scotland, we may further infer that the 'C' type of beaker is subsequent in time to the 'A' beaker.

On the basis of these two lines of evidence I re-assert my belief that the 'C' beakers of Scotland, representing over 97 per cent. of the 'A+C' complex of that country (according to Abercromby), are a subsequent development of British 'A' beakers, and not a fresh arrival from the Continent. I must thank your correspondent once more for the interest shown in a problem which has been on my mind for some time, and especially for drawing attention to the matter of the 'B' hybrids.

### STURGEON IN ANGLO-SAXON TIMES

Dr J. TRAVIS JENKINS, Superintendent of the Lancashire and Western Sea Fisheries Joint Committee, writes in reference to the note on 'A Saxon Fish-pond near Oxford' published in *ANTIQUITY* for December 1930, pp. 480-3 :—

'Although the sturgeon is now a very rare fish in British waters there is evidence that it was abundant in Anglo-Saxon times. In addition to the 'Styrian Pol' of the *Cartularium Saxonicum* there are two references to 'Stirigan Pole' in the fourth volume of Kemble's *Codex Diplomaticus*. This is the first reference I have ever seen to the keeping of sturgeon in ponds, as it is undoubtedly the sturgeon that is referred to. *Porcopiscis* is the dolphin (a mammal), *rombus* is the turbot, but *cragacus* cannot be associated with any known fish. I should think the association of these names with Styria was due to ignorance.

'An extensive search reveals no records of the keeping of sturgeon in ancient or modern times in storage ponds, and I should be grateful for any references to this subject'.



## Recent Events

*The Editor is not always able to verify information taken from the daily press and other sources and cannot therefore assume responsibility for it.*

At Egtved in Jutland an oak coffin has been found containing the body of a girl of the Bronze Age whose face and clothes were in a good state of preservation. This was due to the preservative property of the bark. She had short hair bound with a ribbon ; she wore a brown woollen jacket with long sleeves and no buttons, and a very short skirt kept in position by a fringed belt. Her ornaments consisted of a pair of ear-rings, a necklace, two bracelets and a brooch. At her side was a beer-mug in which traces of grain and honey have been detected. She was in fact a very modern young person (*Rev. Arch.*, xxxiii, 333-4).



We are indebted to one of our readers for the information that at Roknia (near Constantine in Algeria), where a very extensive 'dolmen-field' has been known for a long time to exist, there are also to be found *rock-cut tombs*. It is very much to be desired that plans of these should be made—as also of the dolmens ; and by plans we mean careful surveys like those published in *Archaeologia*, 1927, LXXVI, 121-60, by Mr W. J. Hemp, of the Majorcan rock-cut tombs. Here is an opportunity for post-graduate work.



The Petrie Medal was awarded again this year by the University of London. The award is made once every three years for the publication of outstanding archaeological work in the field. The previous holder was Sir Aurel Stein whose work in Central Asia is famous. On Foundation Day, 28 November, the medal was handed by the Vice-Chancellor to Sir Arthur Evans, of Cretan fame.



Dr Wheeler's courses in British archaeology, held in connexion with the University of London, have been continued during the winter. They include a visit to the Wiltshire downs at Whitsuntide.

## NOTES AND NEWS

In 1913 Dr Hans Reck discovered a skeleton at Oldoway in Kenya. The site has now been revisited by Dr Reck in company with Dr Leakey, and further important evidence of the age of the deposit in which it was found has been discovered. The facts are however too complicated to follow without the plans and sections which will doubtless accompany the detailed report (*Nature*, 12 December 1931).

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In the Pipe Rolls of the 12th century frequent mention is made of the Forest of Halla Haroldi, or Harold's Hall. Up to the present nothing has been known of this forest and hall beyond the fact that they were in the Hundred of East Meon in Hampshire. They are now identified by Mr Herbert Chitty from an unpublished document at Winchester College. In this, Harold's Hall occurs among the bound-marks of the Manor of Huntbourne 5 miles SE of Bishop's Waltham. The forest must therefore have been that later known as Bere. Mr Arnold claims to have located the ruins of the hall near Clay's Copse. The interest of the discovery lies in the fact that the Harold in question can only have been the famous Harold, the last Saxon king of England.

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On the small inset plan of Bath on Speed's Map of Somerset (1610) is marked 'the Boate-stall'; and there is a picture of what seems to be a ferry. A man is crossing the Avon on a raft or punt, holding with both hands an overhead rope swung across the river. On an old plan of Bath of 1572 Boat-stall lane leads down to this ferry. At the mouth of the Parret Speed's map also marks 'Botestall poynt'. One wonders whether 'Boat-stall' meant simply (as it should) a place for boats, a wharf; or whether it had come to have some more special connotation.

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A hut-site of the Saxon period has been found in a gravel-pit beside the Foss Way near Bourton-on-the-Water, Glos. It has been excavated by Mr G. C. Dunning and Miss Donovan, who took part in the excavation of Salmonsbury last summer. It is gratifying to know that, as a result of the interest aroused locally by these excavations, many other antiquities, which otherwise would have been overlooked, have been brought to notice. (*Gloucestershire Echo*, 18 December 1931; *Cheltenham Chronicle*, 19 December 1931, illustrations of pit after clearance; *The Times*, 24 December, p. 9).

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There is no end to the archaeological richness of English soil. We were accustomed to the discovery of ancient Greek coins and statues, of Etruscan urns and such like. But it is nearing the limit when a modern Turkish inscribed tomb-stone is found buried at Kirkley near Lowestoft! (*Eastern Daily Press*, 14 November). The facts are however quite certain, and capable of a relatively simple explanation (*ibid.* 17 November).



The International Congress of Archaeology will be held in London August 1 to 6 next, after which there will be excursions to sites of interest in the provinces. The headquarters of the Secretariat is at Burlington House (Society of Antiquaries).



A small hoard of gold objects of the Bronze Age has been found at Towednack near St. Ives, Cornwall. To judge from the illustrations they consist of two torcs and six rings. The torcs have recurved hooks and appear (so far as one can judge) to be formed of a solid bar of twisted metal, not of the ribbon type. (*The Times*, 2 January 1932, p. 6; *Western Morning News*, 30 December 1931; *Birmingham Post*, 31 December 1931).



Mr J. BUTTER, of Deventer, Holland, has kindly sent us the following note, together with specimens of the grain itself :—

‘ At a depth of one foot in a Celtic field near Emst I found, on the land of the owner Mr G. van ’t Laar an extinct variety of *Triticum vulgare* (Common wheat). The determination of the species is by Dr Florschütz of Velp. This was a deductive discovery. Since Celtic fields existed there I promised a guilde for the first grain found there ; and it was found in January 1928. I excavated there and gave the results to Dr Florschütz ’. The fields were described in *ANTIQUITY*, 1928, II, 85.

The specimens of grain were sent, as likely to be of interest, to a British expert who replied as follows :—‘ It has been the practice for some time past to try and identify prehistoric wheats from specimens of grain alone, but this procedure ignores the fact that carbonization itself induces changes in shape. It has been found, by artificially carbonizing grains, that the changes are normally big enough to make it difficult, and often impossible, to identify the species. There is



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therefore little hope of identifying wheat species unless we can get more than the grain'. With regard to the present specimens he concludes that most of it is wheat of some kind, though amongst it there *may* be a little barley.

This is an unfortunate position for archaeologists, since the more perishable parts of the ear are naturally the ones least often preserved; and it may be no fault of their's that they cannot give the expert what he demands. Every care should however be taken to supply him with, in addition to the grains themselves, specimens of the adjacent soil which may contain the desired fragments. We mention this for the guidance of future excavators.

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An article by Professor G. Elliot Smith in *The Times*, 11 December (p. 15) summarizes the very interesting discovery made by Mr W. C. Pei, the geologist in charge of the excavations at Chou Kou Tien, of stone implements in association with the remains found there of Peking Man, and the evidence adduced by Professor Davidson Black that the use of fire was known even at that stage in the history of man. The first notice in *The Times* of these discoveries appeared on 4 November, and this was followed the next day by a letter from Professor Elliot Smith pointing out their significance.

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The results of the excavations at Herculaneum during the past four years are reported by Professor Maiuri in *The Times* of 6 November (p. 15).

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A summary of the excavations at Paestum is given in *The Times* of 12 January (p. 11), with an outline of a report prepared by Senator Giuseppe de Lorenzo, who does not agree that the ancient city was destroyed by floods, but rather favours a theory that its disappearance was due to a geological movement, which buried it, and that a reverse movement restored the ruins to sight.

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Some remarkable funerary remains have been found by the Archaeological Survey of Egypt in Nubia, south of the Great Temple at Abusimbel, where tumuli of late Roman period built by an Ethiopian people have been opened. In one of these, skeletons of animals, their trappings of silver, harness set with precious stones, and other remains, point to the burial of an important personage. (*The Times*, 11 January).

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Chysauster, a stone-built Celtic village in the parish of Gulval, some three miles from Penzance, is described in a special article in *The Times*, 23 January (p. 11). It was a tin-mining settlement inhabited as early as the 1st century B.C., and continued in occupation for 200–300 years after that time.

Dr R. E. Mortimer Wheeler has contributed a special article to *The Times* (6 January, p. 11) reviewing the discoveries made during excavations on the site of Verulamium.

A very useful addition to our knowledge of Persian history has been made by Mr A. W. Davis, British Vice-Consul at Resht, Persia, who has read and copied the Old Persian superscriptions on the sepulchres of the Achaemenid kings near Persepolis. These are cut above the figures representing the 30 nations subject to the kings of Persia, and supporting a platform on which the kings are shown worshipping before the sacred fire. The particular inscriptions read by Mr Davis are those on the South Tomb, which is ascribed to Artaxerxes II, 404–358 B.C. As Mr C. J. Gadd says in his letter to *The Times* (7 January, p. 6) communicating the discovery, Mr Davis is to be congratulated on recording a feature which has hitherto escaped attention.

The third expedition conducted by Professor John Garstang to excavate the site of ancient Jerusalem is receiving support from the Museum of the Louvre as well as from English sources. He proposes to continue work on the sites of the Royal Palace and the Necropolis begun last season. (*The Times*, 4 December, p. 13).

Sassanian painting and sculptures have been found at Hira, a Neo-Persian site which is being excavated by permission of the Oxford-Field Museum Expedition under the direction of Mr Gerald Reitlinger and Mr Talbot Rice. We hope to have detailed particulars sent to us for publication.

We regret that an error was made in the notes on recent articles in the *Illustrated London News* printed in the December number of ANTIQUITY, p. 501. The chryselephantine figurine there mentioned was acquired for the Royal Ontario Museum of Archaeology at Toronto from a dealer in England.

## Some Recent Articles

*This list is not exhaustive but may be found convenient as a record of papers on subjects which are within the scope of ANTIQUITY. Books are occasionally included.*

THE TEMPLE OF THE WARRIORS, by Earl H. Morris. *Charles Scribner's Sons*, 1931. pp. 251. 21s.

An account of the excavation and restoration of a Toltec-period temple at Chichen-Itza. The work was carried out by the Carnegie Institution of Washington and Mr Morris's volume, very handsomely illustrated, is a record of such achievement as is too seldom attempted in archaeology. It is unfortunate that the author, apparently in an attempt to popularize the subject, should write in a style which vacillates between the levels of a banal journalese and the murky deeps of a hilarious illiteracy. J. L. MITCHELL.

OCEANIA : a journal devoted to the study of the native Peoples of Australia, New Guinea, and the Islands of the Pacific Ocean. *Melbourne : published for the Australian National Research Council by Macmillan & Co., 1930-1931. Nos. 1-4, 7s 6d each.*

The aims of this new periodical are set forth in its sub-title. Its editor, Dr A. R. Radcliffe-Brown, defines the word 'study' from the Journal's view-point as that which is 'properly carried out only by scientists who have been specially trained for the purpose'. Neither his own training nor erudition can be questioned in a lengthy paper, 'The Social Organization of Australian Tribes', which runs the gamut of the first four numbers and dwarfs other contributions in both subject-matter and scholarship. Miss Camilla H. Wedgwood contributes various papers on the secret societies, wars, and other of the less hygienic activities of Melanesia, and is apt to assume, very unwarrantably, underlying rational purpose in savage ritual—while decrying rational thinking in primitive man. Mr A. P. Elkin, in no. 3, has an interesting paper on the 'Rock Paintings of North-West Australia' and neatly burkes the question of their origin, thus avoiding conscription in the ranks of Dr W. J. Perry's adherents. Dr Raymond Firth deals with 'Totemism in Polynesia' uninspiredly but conscientiously. All in all, a very welcome periodical. Some amelioration of the paucity of its illustrations would add considerably to its value. J.L.M.

EL TOKI MAGICO, por J. Imbelloni. (*Anales de la Sociedad Científica de Santa Fe*). *El Establecimiento Gráfico Tomás Palumbo, Buenos Aires*, 1931. pp. 24. Price not stated.

Sr. Imbelloni, pursuing his researches into native American affinities with Polynesia, compares, detail for detail, the words and formulae of the Araucanian



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and Maori 'ceremonials of enchantment' in connexion with the use of the *Toki*, or stone axe. A sketchy, but cautious and scholarly, treatment of a cultural item confirming the obtrusive influence of Oceania in pre-Columbian America. J.L.M.

ON THE DIFFUSION IN AMERICA OF PATU ONEWA, OKEWA, PATU PARAOA, MITI, AND OTHER RELATIVES OF THE MERE FAMILY, by J. Imbelloni. (*Journal of the Polynesian Society*, Dec. 1930). pp. 34. Price not stated.

Details the various discoveries, in North and South America, of examples of the shaped greenstone club or sword which went in Polynesia by the general name of *mere*. Photographs of the various weapons—including an ancient Mexican specimen apparently decorated by a Maya craftsman—are given, the antiquity of the Mexican *mere* by itself disposing of the contention that these weapons were introduced into America by Captain Cook. J.L.M.

WIRA KOCHA. *Revista Peruana de Estudios Antropologicos*. Editor: Julio C. Tello. Vol. 1, no. 1. Enero-Marzo, 1931. Lima, Peru. Subscripción Anual en el extranjero: \$5; numero suelto: \$2.

Sr. Tello's new review is one of both promise and achievement. The editorial note sets forth the policy of the journal as an impartial study from all angles of the ancient Peruvians and their habitat. Among other notable contributions is a paper on the crops and food-stuffs of the pre-Spanish states, by M. T. Mejia Xesspe, and one on the use of the martinet bird in the decorative art of Nasca, by Eugenio Yanowleff. The number is satisfactorily illustrated with photographs and drawings. J.L.M.

Die Langmauer bei Trier und ihr Bezirk—eine Kaiser domäne, by Josef Steinhausen. *Trierer Zeitschrift*, vol. VI, 1931, heft. 2-3, 41-79, 7 plates, sketch-map.

Tardenoisien in der Krim, Neolithikum im Wolgagebiet, by Boris Schukov (Moscow). *Bericht über die hundert-jahr Feier*, 21-25 April 1929 (de Gruyter, 1930), 281-90.

An interesting account of carefully conducted excavations. Records the discovery of pottery associated with Tardenoisian culture in the Crimea which, if contemporary with the western phase, becomes the oldest ceramic of Europe.

Der Goldberg bei Nördlingen und die moderne siedlungsarchäologie, by Dr Bersu. *Ib. id.* 313-18.

A summary of the results of excavation in this important hill-fort which was occupied from neolithic times onwards.

Die Ergebnisse der Warfengrabung, by A. E. van Giffen. *Ib. id.* 322-38.

A useful summary of what is known about these important but little-known Dutch marsh-dwellings.

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The Rise and Progress of Classical Archaeology, with special reference to the University of Cambridge; an inaugural lecture by A. B. Cook, Laurence Professor of Classical Archaeology. *Cambridge University Press*, 1931. pp. 61. 2s.

A useful but necessarily brief outline. The special reference compels the exclusion of certain pioneers not associated with Cambridge (such as Horsley), but allows our old friend Stukeley to appear. We most heartily endorse the plea for the foundation of a Readership of British Archaeology, but should go further and demand a Professorship. British archaeology is the creation of the amateur; but it is now firmly established as a profession, and is badly in need of recruits. There is still, however, no adequately endowed and equipped university school of training; and vacancies in the profession have to be filled from outside sources.

Zur Megalithkultur Nordwestdeutschlands, by Ernst Sprockhoff (Römisch-Germanisch Zentralmuseum, Mainz). Sonderabdruck aus *Nachrichten aus Niedersachsens Urgeschichte* no. 4; 1930 (August Lax, Verlagshandlung, Hildesheim). pp. 55, plans, sketch maps and schedule.

Die Sprache der Festlandkelten, by Leo Weisgerber. *Deutsche Arch. Inst. (Röm.-Germ. Komm.)* 20th Report, 1931, 147-226.

Not the least valuable part of this article is the glossary of old Celtic words on pp. 191-214, which includes words such as briga, -duro, onna, Tamesa, vindo-, of interest to British students. There is a list of references of 272 items.

Ausgrabungen und neue Funde in Frankreich aus der Zeit von 1915 bis 1930 (Paläolithikum bis Römerzeit), by Raymond Lantier. *Deutsche Arch. Inst. (Röm.-Germ. Komm.)* 20th Report, 1931, pp. 77-146.

This is a valuable summary by a specialist of the more important discoveries made in France since 1915. Such occasional 'stock-taking' is becoming necessary in every branch of rapidly advancing knowledge; and M. Lantier's monograph (which from its nature is naturally incapable of further condensation here) will be found indispensable by all prehistorians and Roman students.

Unser Wissen von den alten Kulturpflanzen Mittel-Europas, by Fritz Netolitzky. 20th Report (1931) of the Römisch-Germanische Kommission of the German Archaeological Institute. (Joseph Baer, Frankfurt am Main), pp. 14-76.

A very valuable monograph, illustrated by two sketch-maps (ancient distribution of millet and dispersal-routes). It is quite impossible to do justice to it in a few lines, but it will be found indispensable by serious students. The last five pages contain a list of references arranged alphabetically under authors.

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New views on the relations of the Aegean and the North Balkans, by V. Gordon Childe. *Journ. Hellenic Studies*, L, part 2, 1930, 255-62.

The Religion of ancient Palestine in the light of Archaeology, by Stanley A. Cook (British Academy, Schweich Lectures 1925). Milford, 1930.

Etudes de Stratigraphie paléolithique dans le nord de la France, la Belgique et l'Angleterre, by the Abbé Breuil and L. Koslowski. *L'Anthropologie* XLI, 1931, 449-88.

We postpone fuller consideration of the above until publication is complete. The first part deals with the valley of the Somme only.

Les Hittites, by G. Contenau. *Ib. id.* 489-99.

Le progrès de la paléontologie humaine en Allemagne, by R. Vauflrey. *Ib. id.* 517-51.

A valuable summary of recent work, well illustrated.

Kritische untersuchungen über *Australopithecus africanus* Dart, by W. Abel. *Morphologisches Jahrbuch* 1931, LXV, 539-640. Reviewed in *L'Anthropologie*, XLI, 562-4.

Considerations sur certaines formes caractérisant l'âge du bronze de l'Europe sud-orientale, par Catherine Dunareanu-Vulpe. *Paris, Gamber*, 1930. pp. 60 and 3 plates.

L'âge du fer dans les régions thraces de la péninsule balkanique, par Rader Vulpe. *Paris, Gamber*, 1930. pp. 178 and 6 plates, map.

The Maltese rock-cut tombs of a late pre-Christian type, by Sir T. Zammit. *Bull. of the Malta Museum*, November 1931, I, no. 3, 101-31.

Les souterrains-refuges de Bas-Quercy, by R. Pouch. *Bull. Soc. préh. franç.*, November 1931, XXVIII, 465-69.

Faucilles préhistoriques de Ras Shamra, by G. Chenet. *Ib. id.* 469-75.

Un Glozel espagnol: les falsifications d'objets préhistoriques à Totana, by Juan Cuadrado Ruiz and A. Vayson de Pradenne. *Ib. id.* September 1931, XXVIII, 371-89.

M. Vayson de Pradenne is carrying on the good work of casting light upon the dark places of archaeology, and banishing the evil spirits that haunt them.



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Handbook and Guide to the Aegean and Hittite Collections on exhibition in the public museums, Liverpool, 1931. 3d.

The publication of guides to museums is always to be commended, and this one will be found useful by all visitors. Two minor blemishes should be corrected in the next edition: the typography (especially the title-page), and the book-list, which should give author's initials, the publisher, price and date. Those who may think initials a pedantic demand should turn up, say, Burn, Hall and Evans in the Bodleian catalogue. It might be a good thing also in the next edition to give more prominence to the writer's name which at present is concealed in the preface. The idea of publishing illustrations of the objects in their natural setting is an admirable one: and we are duly thankful for the pleasant little map (though the coast-line of Greece leaves something to be desired).

L'Afrique préhistorique, by the Abbé Breuil. *Cahiers d'Art*, 1930, v, 449-500.

Reviewed in *L'Anthropologie*, xli, 582-4. 'In this important work our knowledge of prehistoric Africa has been for the first time brought together and classified both geographically and (so far as possible) chronologically'.

Staré Osídlenie Slovenska [Ancient Slovak settlements], by S. Janský. *Sborník Muzeálnej slovenskej spoločnosti* (Turčiansky Sv. Martin), 1930, xxiv, pp. 67 and 17 plates; summary in French.

Reviewed in *L'Anthropologie*, xli, 590-1. 'The interest of this monograph, as of the author's preceding one on Slovak *oppida* (see *L'Anthropologie*, xli, 177), is greatly increased by the abundant topographical maps, with physical features shown, by which it is illustrated'.

Neolityczny grob skrzynkowy ze Skoczki, w pow. szubinskim [A neolithic cist-burial (tombeau à caisse), district of Szubin]. Publication d'hommage à W. Demetrykiewicz, Poznań [Posen] 1930. Summary in French.

Reviewed in *L'Anthropologie*, xli, 591, where it is claimed as evidence of megalithic civilization in Eastern Poland.

Lewis Henry Morgan: social evolutionist, by B. J. Stern. *Univ. of Chicago Press*, 1931, pp. 322.

Reviewed in *L'Anthropologie*, xli, 629-31. Contains a bibliography of Morgan's published and unpublished works.

Das Alter des frühgeschichtlichen Gräberfeldes von Ur, by Viktor Christian (Wien) and Ernst F. Weidner (Berlin). *Archiv für Orientforschung*, 1931, vii, 100-12.

A controversial article arguing on behalf of a later date than that assigned by Mr C. L. Woolley.

## Reviews

SKARA BRAE : a Pictish Village in Orkney. By V. GORDON CHILDE, B. LITT. With chapters by PROF. T. H. BRYCE, F.R.S., and PROF. D. M. S. WATSON, F.R.S. *Kegan Paul, Trench, Trubner & Co.*, 1931. pp. xiii, 208, with 60 plates and 24 figs., plans and sections. 31s 6d.

By reason partly of its subject and partly of its own intrinsic value, this superbly illustrated volume is destined to figure as a standard work in British archaeology. In the Skara Brae settlement, dating in the author's opinion from about 500 B.C., are preserved the remains of at least 10 stone huts, the corbelled walls of which in some parts still stand to a height of about 10 feet. Since nowhere else in these islands is there known to exist a group of prehistoric dwellings in anything like the same state of preservation, their importance is obvious. And in comparison with its merits the defects of the book are so relatively unimportant that they may be dismissed very briefly. The most serious is a certain lack of lucidity—partly in expression and partly in classification. The distinction between 'structural periods' and 'occupation periods' is not kept sufficiently clear. It ought, of course, in justice, to be added that the buildings are so extraordinarily complex in levels and shape and grouping that a perfectly clear and simple description is well nigh impossible. Prof. Childe is aided however by what he justly designates the 'magnificent plans' prepared by Mr Houston of H. M. Office of Works (though it should be noted that at least one wall described in the text as 'd' is not found so designated on the plan). The spelling also of some of the proper names is occasionally at fault, and with reference to the so-called 'Lansbury Gallery' one may slightly modify the phrase to say 'que diable allait-il faire dans cette galerie'?

Taken as a whole, however, the volume is a monument of rare archaeological skill and insight. On the earlier part, which is devoted to a description of the site and the chronological sequence of the various structures, there is little need for comment; the facts are set out carefully, if not always quite perspicuously. But in the later chapters, which deal with the innumerable relics of stone and bone (no metal relics occur)—their nature and relationships—and the contingent dating of the settlement, it becomes ever more and more clear how fortunate was the choice when Prof. Childe was selected to superintend the exploration of this site. For the problems emerging are so unusually puzzling, yet of such general archaeological significance, that they could not have been adequately treated except by a man of the wide experience, scholarship and knowledge of general European archaeology possessed by the author.

Not that one can accept all his conclusions without demur. He says *e.g.* (p. 97) that no artifact 'indubitably worked with metal tools has been recognised at the site'. But among the relics are some thin perforated discs of bone or ivory which could hardly have been sawn without the use of metal; certainly none of the flint tools discovered appear capable of performing the operation. In a paper read before the Society of Antiquaries of Scotland in December 1930, Mr J. Graham Callander, Director of the Scottish National Museum of Antiquities, out of his unrivalled knowledge of Scottish prehistory, subjected Prof. Childe's evidence for a pre-bronze dating of the site to a most

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powerful and incisive criticism. But after careful study the present writer is bound to confess that he agrees with Prof. Childe, whose review of the relationship between the culture of the brochs and that of Skara Brae is a piece of singularly cogent, impressive and convincing argument. The total absence at Skara Brae of the most typical broch relics simply cannot be set aside.

Space forbids more than the barest mention of the amazing wealth of strange relics unearthed—the carved stone balls, the various other stone objects unique in form and workmanship, the Skara Brae bone ‘adze’, the wall-carvings and the rest. But in conclusion one must refer to the curious rarity of pig bones among those submitted to Prof. D. M. S. Watson for identification. One is forcibly reminded of Prof. W. J. Watson’s theory as to the origin of the name—Orkney. Is the comparative absence of pig bones at Skara Brae due to the fact that the inhabitants were of the ‘Orc’ or Boar-folk tribe from whom Orkney takes its name, and that the boar was the tribal totem?

H. MARWICK.

LE PROBLÈME DES CENTAURES. *Par* GEORGES DUMÉZIL, D. ès L., Prof. d’histoire des religions à l’Université de Constantinople. *Paris: Geuthner, 1929. pp. viii, 278. 75 francs.*

This study of comparative mythology deals partly with folklore, partly with some modern survivals of primitive seasonal rituals, and attempts, not without success, to trace a connexion between the two. The author’s first task is to describe traditional masquerades in which horses play a part, and he finds that the ‘Gody’ of Slavonic countries best preserve the form and sense of the ancient rites connected (a) with certain seasons of the changing year, and (b) with marriage. It is not necessary for us to go so far afield for a similar custom: the ‘Mary Llwyd’ exists at the present day in the Gower peninsula, where at Christmas time a party of mummers goes from house to house, one of them wrapped in a cloth and carrying above his head a horse’s skull so arranged that the jaw can be moved by a cord.

Dr Dumézil collects analogous customs from the most remote parts of the world. The next thing is to examine myths of Indo-European peoples which present striking resemblances to the scenarios described in the opening chapters. After commending Mr J. C. Lawson for his identification of the modern Greek Kallikantzaroi with the Centaurs, the author goes further and finds that the Centaurs have relations in all Indo-European mythology, and the culmination of the work is the discovery of the close affinity of the Gandharva of Indian epic to the Centaurs of Greece, and the association of the former with the God of the New Year.

We may not follow Dr Dumézil in all his inferences: on the linguistic equation of *Gandharva* with *Kentauros* M. Meillet suspends judgment; but the book is interesting in itself and contains an invaluable store of references to the literature in all languages bearing on the subject.

J. F. DOBSON.

WEALDEN IRON: a monograph on the former Ironworks in the counties of Sussex Surrey and Kent. *By* ERNEST STRAKER. *G. Bell and Sons, 1931. pp. xiv, 487. With illustrations. 15s.*

That undulating, wooded plain, known as the Weald and forming parts of Sussex, Surrey and Kent, shares with the Forest of Dean the distinction of having been the principal scene of the iron industry of this country from prehistoric and Roman times



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down to the 18th century. The comprehensive study of this industry has been long overdue, and this lack is now amply supplied by the volume before us. It is probably the most complete account of ancient processes of smelting that has yet appeared and should be studied by all who in the excavation of ancient sites elsewhere find traces of this industry.

The preface contains a good epitome of the history of the Wealden iron industry. 'Unlike that in other districts, it was never able to substitute coal for charcoal as its fuel, and so came to a complete and definite end, not, as is popularly supposed, by the exhaustion of the woods, but from economic causes. Commencing in prehistoric times . . . the Romans, soon after their advent, exploited it on a large scale . . . After their departure, during the Dark and Middle Ages it appears to have declined in importance, but remained as a limited and almost domestic industry. In Tudor times, about the close of the 15th century, a new process was introduced from the Continent, and soon after the casting of iron cannon, at first by the help of foreign experts, was commenced in Sussex . . . In our island this was the first step of the change from a practically self-supporting and mainly agricultural community, exporting their surplus produce in an unmanufactured state, to a nation depending for the greater part of its sustenance on manufactured exports, and was intimately connected with the rise of overseas trade and colonisation' (p. vi).

Successive chapters deal clearly and ably with every aspect of this story. That on the geology of the Weald defines the limits of the district and the rocks, ferruginous and otherwise, of which it is composed. This is succeeded by a most important chapter in which the direct, or bloomery process of smelting is distinguished from the indirect, or blast-furnace, process, which was introduced about the close of the 15th century. The former, which was the only method in antiquity and which is still practised by native tribes, produced small pieces of wrought iron without melting the ore completely. By the later process large masses of wrought iron are produced by submitting 'sows' of cast iron to the action of large mechanical hammers, the latter being worked by water-power in the Weald.

Cast iron was unknown before the introduction of this process, unless accidentally produced. The kinds of slag, or cinder, resulting from these two processes are distinct and recognizable, and this is of great value when tracing sites in the field.

The early history is then outlined—early Iron Age, Roman, Saxon and early Medieval. Four pre-Roman and about ten Roman sites have been identified by the associated pottery. In the Wealden hill-fort of Saxonbury slag was pounded up for mixing in the paste of La Tène III pottery, and pieces of slag have been found in other Iron Age hill-forts. Some of the Roman works were exploited on an enormous scale; the cinder-heap at Beauport Park, near Battle, having originally been about 50 ft high, and extending over two acres of ground. When this material was removed between 1870 and 1880 for road-making considerable quantities of Roman pottery were found in the mound, including Samian ware, and also coins of Trajan and Hadrian, and other objects. At Oldlands, in Maresfield, very large quantities of slag were found, together with much pottery, some bearing potters' marks, and also coins of Nero, Vespasian, Tetricus and Diocletian, those of Vespasian being in the majority.

The evolution of the blast furnace is next described, largely from contemporary sources. The next three chapters trace the history of the industry from Tudor times to its extinction early in the 19th century. Then follow chapters giving more detailed accounts of the technicalities of the industry—hammer-ponds, furnaces, forges, mining

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and ore, fuel, and, most important from the point of view of field archaeology, cinders and slags. Bloomery slags are classified into seven types, illustrated by numerous photographs and micro-sections, and forge slags into two types; chemical analysis being given of each. The differences between the types is to a large extent due to slight differences in the processes of smelting.

Further chapters deal exhaustively with the products of the industry—the casting of guns, fire-backs and grave-slabs—the introduction of steel, the methods of transport, and the all-important question of finance.

One cannot conceive how the subject could have been dealt with more comprehensively or more readably. There is, however, one very disappointing chapter, which is unfortunately the first in the book. Though entitled 'Iron in General', this chapter is almost wholly given up to arguing that iron was known and worked, not only in the Bronze Age, but also in the neolithic period. That the use of iron was known at a very early period in Mesopotamia and Egypt is undoubted, but to apply such a view to Britain is quite another matter. The author puts forward no evidence of the use or knowledge of iron in the British Bronze Age, but argues that it must have been used because it is easier to work than bronze, and he accounts for its absence in Bronze Age deposits by supposing that it has rusted away! Such arguments need no comment. But the neolithic period suffers even greater misrepresentation. 'During the last few years', he says, 'evidence has been fast accumulating that iron was smelted in Sussex in neolithic times'. This astonishing statement is supported by three pieces of 'evidence'. (1) The discovery of iron slag by the present reviewer in South Down Camps is cited; but in each case it was found in Early Iron Age deposits, not neolithic.\* (2) Mr J. E. Ray is stated to have found flint implements at bloomery sites; we also have found beer bottles at Cissbury, but never supposed that the one dated the other. (3) The discovery of several pieces of bloomery slag in the excavation of a neolithic settlement by Mr H. J. Cheney near Rye appears to be more formidable evidence. We have therefore looked into the matter personally, visiting the site in company with Mr Cheney, and reviewing the finds. The pieces of slag were found in a neolithic occupation stratum with definite neolithic pottery, under about a foot of top-soil. But in the same layer were found a dozen or more shards of equally definite medieval pottery which we would tentatively assign to about A.D. 1200. This therefore is the explanation of the slag, which, with the medieval pottery, is an intrusion into the neolithic occupation layer. Enough has been said to show that this chapter is crying out to be re-written, or omitted altogether.

The second half of the book consists of a topographical and descriptive survey of all the known iron-working sites in the Weald, arranged in 21 chapters according to river basins and their tributary streams. This is illustrated by 9 double-page maps (half inch to the mile) by John Bartholomew and Son, Ltd., overprinted in red and blue to show the positions of furnaces, forges, bloomeries, Roman bloomeries, and the average rainfall for 34 years. All this is extremely well done and valuable.

The illustrations, which are very numerous, mostly consist of photographs of sites as they appear at the present day. Some of these are full-page reproductions, but the great majority are reduced to about  $2\frac{3}{4}$  by  $1\frac{1}{4}$  inches, which not only detracts from the sumptuousness of the book, but does not always do full justice to the subjects represented.

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\* The reviewer must take his share of blame for having ignorantly described as 'slag' certain pieces of burnt pyrites found in neolithic levels at the Trundle in 1928.

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We feel that greater value for money might have been obtained by reproducing only half the number of photographs, and making them each about twice as large as they are.† Taken as a whole the book is admirably produced, and should be a standard for others to emulate. The type is clear, three different founts being used for the text and different classes of quotations. The adoption of marginal references instead of footnotes is refreshing, making reference easy, the reader's progress not being hindered by footnote numbers in the text. There are several minor misprints, all unimportant, so far as we have observed them. There are also a glossary of local terms, an index of parishes, and a very complete general index. Finally the cover is fully in keeping with a book which it is a pleasure to possess. Every dweller in the Weald should do so, as well as every dweller within motoring distance thereof, not to mention every lover of the history of England's greatness.

E. CECIL CURWEN.

**SCHLIEMANN OF TROY**: the story of a goldseeker. By EMIL LUDWIG. *With an introduction by SIR ARTHUR EVANS. Putnam, 1931. pp. 336. 21s.*

Schliemann was one of the great discoverers of the 19th century, if not of all time, and yet in the forty years that have elapsed since his death no account of his life has appeared although it was well known that he left the most copious material behind him. An autobiography in the introduction to his *Ilios* is the only connected account of his life hitherto available. Now his widow, son, and daughter have placed his diaries, journals, and letter books at the disposal of Dr Ludwig, who has added Schliemann to his series of biographies of Napoleon, Bismarck, and other heroes. He has painted a wonderfully vivid picture of the self-made business man who made himself also an archaeological excavator. Schliemann's energy, industry, adaptability, amazing capacity for learning languages, careful attention to detail and to method made him almost an ideal man of affairs. His other characteristics, more prominent perhaps in his private life, which is tactfully handled by his biographer, seem to have influenced his business less than his archaeological career. He could be impulsive, impatient, ready to take offence, and yet at the same time generous, romantic, considerate, and loyal. Here in his business and in his private life Dr Ludwig's account is based on documents generally inaccessible and written in a dozen different languages, and must be accepted especially since it is authorized by Schliemann's family. Between the main outlines already known the author fills in the details with strong contrasting colours. Of special value is the charming portrait of Madame Schliemann, who in the author's sympathetic treatment appears in her right place as the true and loving partner of her famous husband's work, joys, and sorrows.

On the archaeological side, however, where he can be tested by independent evidence, the author is less happy and hardly does justice to Schliemann's achievements. Nor does he seem, to me at least, right in considering 'gold' as the mainspring of his life. Schliemann's passion for Homer and his dream of establishing the historical basis of Greek heroic legends by the unearthing of material archaeological facts seem inconsistent with the author's 'gold' motive. He hardly appears to have been a mere treasure hunter, or one of those archaeologists who are credited with digging mainly for loot. If he had been, even in spite of his romantic tendency he could hardly have written 'nothing more

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† We whole-heartedly agree. When *will* publishers learn to prefer quality to quantity in illustrations?—EDITOR.



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interesting can be imagined than the excavation of a prehistoric city of immortal glory where nearly every object, even to the fragments of pottery, reveals a new page of history'. In his publications on Mycenae and Troy there is no excessive jubilation over the gold as gold. In laying emphasis on the archaeological value of pottery he was anticipating modern scientific archaeology. He was looking much more for Homeric parallels and for proof of the historical value of Greek tradition. Here again modern ideas have justified his attitude. The author, in saying that at Mycenae Schliemann removed the grave stelai without determining their positions or their relationship to the graves, is not quite correct. For four years during the recent British excavations at Mycenae I had the opportunity of reading Schliemann's account of his excavations again and again on the actual scene of his labours, and also, thanks to Madame Schliemann's kindness, was able to make free use of his original notebook written from day to day in 1876. From such personal observation and from the results of our own investigations on the stratification of that complicated site, the Grave Circle at Mycenae, Schliemann appears as an excavator in most respects no worse, and in several better, than his contemporaries who had had archaeological training. His businesslike common sense and his acuteness in observation led him to make notes on the stratification in a manner which was not followed by others in excavations of a decade or more later. In his publications Schliemann gave sections, noted the depth at which he found the individual objects, and also gave the scale of his reproductions, points often overlooked even today by archaeological professors who attempt excavation. Schliemann's observations on practical points of this nature should be given serious consideration. At a time when the excavators of Olympia were content not to explore whether the Heraeum was really the earliest temple on that spot, he acted on the principle of digging to virgin soil to find the whole history of a site. If in this he was apt, as in the great north trench at Troy, to be too drastic, his method was not much worse than some of those employed in this century by professional archaeologists. Impatience and impulsiveness, Schliemann's two worst faults as an excavator, are not peculiar to him alone. He and his collaborator Dörpfeld were right in recognizing the palace at Tiryns as a prehistoric building, against the contrary opinion of Penrose. Here his common sense and reliance on the facts of excavation were fully justified. The absurd criticisms expressed about the treasures at Mycenae or by Bötticher on the character of Troy have gone the way of similar futilities.

Thus, in a book so fascinating as this biography of the errand boy who became a merchant prince, and then the discoverer of the great Bronze Age civilization of the Aegean, one could wish that the author had enlisted archaeological aid to enable him to give in parts IV and V the proper value to Schliemann's work as an excavator in the light of the most modern research on Mycenae, Tiryns, and Troy. A. J. B. WACE.

**THE SCULPTURE OF THE NIKE TEMPLE PARAPET.** By RHYS CARPENTER, with photographs by BERNARD ASHMOLE. Cambridge, Mass.: Harvard University Press, 1929. pp. 83, with 34 plates, 15 figures, and plan. \$2.

Designed by the architect of the Parthenon, for twenty-one centuries the little temple of 'Wingless Victory' stood intact upon the bastion of the Acropolis. Pulled down by the Turks to make a bastion of their own, it was reconstructed by Ross in 1835, and is now familiar to all who visit Athens. Surrounding it on three sides there used to be a low parapet, or 'balustrade', carved with figures of Victory and surmounted

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by a metal grille. The fragmentary remains of this parapet have received much attention from scholars—Kekulé, Heberdey, Dinsmoor especially—and are very familiar to students of Greek sculpture as masterpieces of the late fifth century. They illustrate strikingly that mastery in the representation of drapery and in the creation of the illusion of transparency which is characteristic of their period: the most famous of these fragments is that which shows a Nike unbinding her sandal. Professor Carpenter in this book gives us a definitive study of his own researches, which may be said to have crowned the work of those who have preceded him with all but finality: 'all but', since minor problems remain, and there is still hope that a few more fragments may yet be found. Professor Ashmole's share in the work is such as those who know his skill in this kind of photography will expect: and the printing was entrusted to the Spammersche Buchdruckerei, of Leipzig.

Professor Carpenter makes a close stylistic analysis of the fragments, in the order in which they stood; this is illustrated by the photographs showing not only individual slabs, but also details of the technique upon which the conclusions are based. It is thus possible for the reader not only to see for himself the justice of the conclusions, but also to enjoy a lesson in criticism whose value it is difficult to over-estimate. The balustrade is shown to have been the work of six sculptors, whose unequal merit is assessed by reference to their preference for chisel or drill (the use of the 'semi-running' drill became common at this time), their temperament and their command of technique. He suggests, with probability, that they divided the work equally, except that the chief of them made in addition to the half-side which fell to each man's share the little extra portion which forms a short return upon the eastern flank: he thus carved ten figures, each of the others eight. Professor Carpenter wisely prefers to leave these sculptors nameless: but he suggests that in Master 'A' we may recognize the over-anxious Kallimachos, whose sheer skill and passion for perfection tempted him to over-elaboration, as Pliny tells us; while Paionios of Mende, the maker of a yet more celebrated Nike, is most plausibly suggested, by a detailed and illustrated comparison of the drapery of the two works, as identical with Master 'B', and the affinities of Master 'E' with the originator of the Venus Genetrix type are again demonstrated by comparative photographs. But in fact names are of secondary importance; we have the sculptures, sadly fragmentary indeed, but telling their own tale.

W. L. CUTTLE.

THE TREASURIES OF THE GREEKS AND ROMANS. By H. N. COUCH.  
*Menasha, Wisconsin: The George Banta Publishing Company, 1929. pp. 111.*  
*Cloth 10s. 6d., paper, 6s 6d.*

The writer discusses everything that the Greeks and Romans called 'treasuries'; strong-rooms, *tholos* tombs, prisons, money-boxes. His manner is somewhat laboured: so much proof is not required to convince us of the sepulchral use of the Mycenae *tholoi* (for his treatment of which Sir Arthur Evans' latest contribution to the problem of these and the Shaft-Graves' interconnexion had not yet appeared), even if Pausanias does call them treasuries. The fact is that if we translate the word 'keep'—a rendering which agrees well enough with the conservative derivation from *τίθημι* to which the writer adheres, while wisely reserving the right to think that there may be a pre-Hellenic history behind it—we shall not be far wrong. Too much significance is attached to the partial burying of the *pthos* in which Eurystheus is shown as imprisoned: that is the way to use a *pthos*. And when Apollodorus says it was a bronze *pthos*, he is only

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behaving in the traditional epic manner, which exaggerated the bronziness of the Bronze Age.

The latter part of the book, which deals with temple treasuries and private safes, includes a useful discussion of the nature of the *opisthodomos* in which the Athenian public wealth was stored.

The pursuit of this word is interesting; I have been curious about the 'coin-in-the-slot machine which sold holy water' ever since I noticed that meaning in Liddell and Scott, but this Thesaurus of Thesauri leaves me still curious on that particular point.

W. L. CUTTLE.

HOLT, DENBIGHSHIRE: the works depôt of the twentieth legion. By W. F. GRIMES. *Y Cymmrodor*, vol. XLI, pp. 235, 80 figs. Issued by the Society, New Stone Buildings, 64 Chancery Lane, 1930.

In 1907 the late T. A. Acton, F.S.A., undertook, at his own expense, the excavation of what proved to be the pottery works-depôt of the 20th Legion at Holt, about 10 miles from Chester. The work was continued until 1915. Ten years later no report had appeared, when the material was acquired by the National Museum of Wales. The arrangement whereby Mr Acton was to have worked over it at Cardiff was upset by his illness and death in 1925. The task of preparing the report was then undertaken by Mr W. F. Grimes. The difficulties to be overcome were very great. Not only was it found impossible to trace any notes made by the excavator, but there was an almost complete absence of labels on the very numerous finds or of records of their stratification; even some of the original plans seem to have disappeared. Let it be said at once that Mr Grimes has to a remarkable degree surmounted these difficulties and has saved from oblivion a site which is unique in Roman Britain.

The meagreness of the evidence has naturally affected his treatment. The pottery which should have provided a dated series of prime importance has had to be classified on typological grounds alone. The description of the buildings is necessarily lacking in detail. Their main features are, however, clearly described and illustrated by excellent plans and sections drawn by the author. They consist of a workman's barracks, a bath-house, and a small corridor-house. More important are the industrial buildings—two sets of workshops and a drying-shed, a large double-flue pottery kiln, and the main plant, consisting of a range of five (originally six) tile kilns and two pottery kilns. The bath house contained a hypocaust of unique type, shallow channels in the floor being formed of a triple row of box tiles. If these really were flues and not ventilating channels similar to those recently found in the tepidaria at Mumrills, they must have been so inefficient as to be only explainable as the work of an amateur architect under orders to make use of a number of unwanted box tiles!

The dating of the activities of this industrial plant is vaguer than it would have been, had proper records of the excavation survived, but it seems clear that it was established towards the end of the first century, to provide the tiles required for the re-building in stone of the legionary fortress of Chester. Its occupation was intensive during the early second century, fell off during the Antonine period, when the military forces of Wales were transferred to the northern frontier, and saw a revival of activity early in the third century. The date of its final abandonment cannot be fixed.

Section IV of the report deserves special attention. In this the author has attempted an illustrated classification of Romano-British kiln types, followed by an annotated



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topographical list of kiln sites. Until recently the meagre and often inadequate records of such sites were buried in the journals of local archaeological societies. The future investigator of this hitherto neglected branch of Romano-British industry will have cause to thank Mr Grimes for his pioneer work.

The report is beautifully produced and printed and admirably illustrated by good half-tone blocks from photographs taken during the excavations and by numerous drawings by the author, which are models of what such drawings should be. The thanks of all students of Roman Britain are due to the Honourable Society of Cymmrodorion for a publication worthy of so important a site and to the writer for so ably surmounting difficulties that should never have arisen.

PHILIP CORDER.

THE ROMAN FORT AT MUMRILLS NEAR FALKIRK. By Sir GEORGE MACDONALD and ALEXANDER O. CURLE. *Proc. Soc. Antiquaries of Scotland*, LXIII, 396-575.

This valuable report gives the result of work carried out during four and a half years on the fort at Mumrills, on the Antonine Wall. In 1923 the site was included in a housing scheme, its examination thus becoming urgent. The illustrations show that the work was one of peculiar difficulty, but the skill and patience of the excavators have not only recovered the history of the site, but, owing to their discoveries in connexion with the two bath-houses, have produced a report that is indispensable to the student of Roman Britain.

The history of the site begins with a short Agricolaan occupation to the west of the later Antonine fort. This early fort, of which slight but convincing traces were found, had the unusually large area of 6 acres, more than ten times that of the contemporary fort at Bar Hill, which suggests that it may have formed the headquarters of the officer in command. The Antonine fort is also much larger than any known station on the Antonine Wall. That the commandant's house, at first a large wooden building, was reconstructed, after being burnt, on a more ambitious scale in stone, suggests that the fort was once again selected as the headquarters of the officer in command of the Wall. Three periods were distinguished in this and in the headquarters building, confirming the generally accepted view that the Antonine Wall was twice destroyed before its abandonment. The relative scarcity of finds on this site, in contrast with the rich harvest at Newstead, is explained by the hypothesis that the final withdrawal of the garrison was here deliberate and orderly.

Two bath-houses were found within the ramparts—a small building in the northeast corner, probably for the use of the men, and a larger establishment that formed the southeast corner of the commandant's house after its reconstruction in stone. The section of the report dealing with the hypocaust arrangements in these two buildings is an original contribution to Roman studies of first rate importance. The discussion of the probable mode of operation of the channelled hypocaust in the men's bath-house (p. 460), and of the ventilating channels beneath the pillared floors of the tepidaria and caldarium in the large bath-house (p. 483) will form a guide to all future excavators and will lead to the reinterpretation of many past excavations. Indeed his discoveries have already led Sir George Macdonald to re-examine the bath-house of Chesters, the first intelligible account of which he gives in the current volume of *Archaeologia Aeliana*.

The authorship of the report is sufficient guarantee of its general excellence. In some respects, however, some of the illustrations are unworthy of the text, with its

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lucid exposition aided by the excellent plans. The half-tones, from photographs clearly taken in bad weather conditions, are too often poor (figs. 22, 59 and 65, for instance). Opinions will continue to differ as to the best method of illustrating decorated Samian ware. The pencil drawings here used have great artistic merit, but nothing can be said in favour of the so-called 'elevational' drawing of a Dr. 37 (fig. 77, no. 8), in which it is represented as a Dr. 30. Much of the coarse pottery, though shown in section, is drawn in perspective, so that the exact slope of the side of a vessel is not certainly indicated. When this method is combined in a single drawing (fig. 105, no. 9) with the 'ideal section' method, now universally adopted in good excavation reports, faith in its accuracy is severely shaken.

PHILIP CORDER.

EXPLORATION ARCHÉOLOGIQUE DE DÉLOS: faite par l'École française d'Athènes. Fascicule XI: les sanctuaires et les cultes du mont Cynthe. *Par* ANDRÉ PLASSART. *Paris: E. de Boccard. pp. 320 with 6 plates and 257 figs.*

Every Hellenic traveller who visits Delos climbs Mount Cynthus to enjoy the panorama of the island and of the Cyclades to be seen thence and also on his way to study the sacred grotto long supposed to be the birth-cave of Apollo and his twin sister. Those who have seen Delos more than once in recent years will have remarked with satisfaction the methodical progress of the French archaeologists towards a complete exploration of the whole mountain. This has now been achieved by Professor Plassart and in this volume, which is in scholarship, printing, and illustration worthy of its subject, he describes and discusses in detail the results obtained by himself and his colleagues. The text is clear and concise, and he adds much valuable material to our knowledge of Delos from all points of view, archaeological, historical, and religious. The only thing lacking is a full index. The author begins with the summit and its first inhabitation, which dates as far back in the Bronze Age as the last centuries of the third millennium B.C. There are house foundations, stone implements, obsidian flakes, and fragments of Cycladic and Mycenaean pottery. He proceeds with the sanctuaries of Zeus and Athena which next occupied the summit, and describes them chronologically through archaic times and the periods of Delian independence and of the second Athenian domination. He then comes to the western slope and the extremely interesting precinct of Hera. Here a small seventh century shrine was in the sixth century completely surrounded and hidden by a small marble Doric temple, distyle in antis, which is worth attention for the slender proportions of its architecture. In the early temple thus buried was found a wonderful hoard of archaic votives of all kinds, some of which make the identification certain by their inscribed dedications to Hera. Of this important collection a special account is given in a separate volume by M. Dugas. Before the temple stood an altar. A road which leads up the northwest side of the mount approaches the sacred cave. After a long and careful discussion of all the circumstances the author concludes that it was a shrine of Heracles and according to the pottery, sculpture and other finds dates it to the Hellenistic age, probably the third century B.C. He is unable to find any definite evidence that it was a primitive or archaic sanctuary which had been restored and reused. The last sides of the mount to be described are the northern and eastern, where with some nameless sanctuaries are those of the Gods of Ascalon, of Zeus Hypsistos, of Eileithyia and the rock of Leto. In a short summary some of the more important points are indicated. First comes the prehistoric age with the traditional connexion of Minos with the islands and of the

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'Carians' with Delos. The sanctuaries on the sacred mount which gave Artemis her favourite epithet Cynthia prove the popularity of Delos in the Greek world and the inscriptions and other dedications show how widespread was the reputation of Apollo's island when in the Hellenistic age the bounds of that world were enlarged. It was honoured by Ptolemy Philadelphus and Arsinoe. The shrine of the gods of Ascalon, and votives from citizens of Gerrha, Seleuceia ad Tigrim, Gaza, and Iamneia attest the fame of Delos in the Levant. Zeus Hypsistos may well have been Baal and an Arab from the Hadramaut engraved in his own language a dedication to the moon god. Thus a study of Cynthus gives an epitome of the history of one of the great panhellenic shrines and the far reaching influence of Greek culture in Hellenistic days at least.

A. J. B. WACE.

ARCHÄOLOGISCHE ENTDECKUNGEN IM 20 JAHRHUNDERT. By FRIEDRICH VON OPPELN-BRONIKOWSKI. Berlin: Heinrich Keller, 1931. pp. 165, with 40 illustrations. 4.50 Rm.

This attractive little book is intended as a supplement to the well-known work of Michaelis, *A Century of Archaeological Discovery*, published in 1903. It is designed to give the German public an idea of the extent and nature of the archaeological discoveries which have been made since the beginning of the present century. The author modestly disclaims the title of 'professional archaeologist': but he has travelled widely, and he speaks from first-hand knowledge. As might be expected in the work of a German, writing for Germans, most of the researches which he describes are those of German excavators: but by no means exclusively so. Full justice, for example, is given to the work of Woolley at Ur. (By a slip, the initials of his lamented predecessor H. R. Hall are reversed). The five chapters of the book are devoted in turn to Mesopotamia, Syria, Asia Minor, Egypt, Classical Archaeology, the Romans in Germany, Prehistoric and Early Historic Germany. For the English reader, perhaps, the last two chapters are the most useful; the sensational discoveries made in the great civilizations of the near East are rendered familiar by easily accessible publications; but the wonderful work which German scholars have recently accomplished, in elucidating the Archaeology of their own land, has to be sought for in a mountain of periodical literature. The bibliographical references given in this book will aid the search considerably.

A few of the illustrations, representing the restorations of ancient buildings, are perspective drawings; the rest are all reproductions of photographs. One of these, that of the temple of Zoser at Sakkara, seems to be slightly over-exposed, and in consequence is a little flat; but the rest are uniformly excellent, both in their photographic printing and in their reproduction as half-tone plates.

R. A. S. MACALISTER.

MERCHANT VENTURERS IN BRONZE. By HAROLD PEAKE and HERBERT JOHN FLEURE. Oxford: Clarendon Press (Milford), 1931. pp. vii, 168, with 67 illustrations. 5s.

This is the seventh, and in many respects the most interesting, of the invaluable 'Corridors of Time' series, which is by now well established as the most convenient short summary of cultural history available in English. Parenthetically, the 'hole-and-corner' suggestiveness of the word 'corridors' always strikes at least one admirer of the series as misplaced: 'Broad Highways of Time' would seem to him a more appropriate designation!



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None of the volumes of the series is an 'arm-chair' book. The amount of material compressed into their narrow compass is astonishing, and as an inevitable consequence, very close attention is needed in studying them. The present volume is worthy to stand beside its predecessors; and this is high praise.

Its subject, generally speaking, is the Bronze Age. The authors convey their readers in a swift survey of the field from Ireland to China: the student who diligently follows them will have laid a sound basis for an accurate knowledge of the period, and for an understanding of the processes of reasoning whereby forgotten, unrecorded history can be recovered from uninscribed relics.

Being a human production, the book is not wholly faultless. The illustrations, on the whole, are the weakest feature. They are well selected, but some of the drawings are scratchy, and certain of the photographs (especially those on pp. 39, 145) are printed rather too black for clearness. Photographs would have been more satisfactory than the not very pleasing drawings of ethnological types on pp. 5, 11. Surely a better likeness of a broad-headed German could have been obtained than the old woodcut—little better than a caricature—of Martin Luther: and we doubt the desirability of inserting named portraits of well-known living persons, and turning them into ethnological specimens. On p. 3 we note the old mistake of treating Ofnet as a *place* ('from Ofnet in Bavaria'). The word, which literally means a baking-oven, is in the local patois applied to a rock hollow: the Great and the Little Ofnet are *caves*, and should be referred to as such. The La Tène collar from Brichter should not appear on a plate with a caption assigning the objects figured upon it to the Bronze Age (p. 21). The fanciful name 'sun-disk' applied to another object on the same plate might with advantage be quietly dropped. There is no sense in it, and the analogy with the Trundholm disk, which has suggested it, is wholly superficial.

On the other hand the book contains many good points, among which we may mention the very probable relation suggested between the tholos graves and the shaft graves of Mycenae; the recognition that there is no water-tight compartment between the Neolithic and the Bronze Ages; the emphasis laid upon the 'secretiveness of inventors' as a source of complication in cultural history; and the admission of the possibility of independent inventions in different countries—a welcome ray of hope that we are at last being delivered from the Pan-Egyptian nightmare.

R. A. S. MACALISTER.

THE HISTORY OF THE MAYA. By THOMAS GANN and J. ERIC THOMPSON.  
*Charles Scribner's Sons, 1931. pp. 264. 8s 6d.*

The collaborators in this history have hardly attempted coordination, as they confess. Some of the chapters Dr Gann wrote in Belize, the while, remotely in Chicago, Mr Thompson wrote others. As result, there is occasionally a refreshing disagreement—one author blandly and unwittingly refuting the statements of his colleague's previous chapter. Variety—if not gaiety—is added at the expense of conciseness.

Dr Gann traces the origins of the Maya civilization to the culture of the 'Archaic horizon' first defined by Dr Spinden. Modern research has shown both culture and 'horizon' as more imaginative concepts than definitions. Bringing the Maya colonizers of the Old Empire from Huastlan Dr Gann also marches with the orthodox, though there is no trace whatever of such a migration. To site the Tuxtla statuette is irrelevant: it may well have been an export from the Old Empire region. And, while among

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irrelevancies, it may be pointed out that there is the best of traditional evidence on the side of the assumption that the Teotihuacan pyramids are Otomi, not Toltec work.

Dr Gann gives a brief sketch of the coming into being of the Old Empire, its arts, crafts and sciences. He disbelieves in any sudden catastrophic evacuation of the historic territory, holding that Yucatan had meantime been fairly thoroughly colonized and that the drift of the Maya into that territory was no unpremeditated exodus. The Bacalar region apart, there is still no evidence of any such colonization, for neither the Cobá nor Tulum dates can be ascribed with any certainty to the tenth cycle.

Mr Thompson outlines the history of the New Empire, and, apart from the usual confusion of Xiu with Itza, the outline is of value.

The accounts of the religious ceremonies, daily life, war organizations and calendrical attainments of the Maya are admirable, and, like the earlier sections, skilfully illustrated.

J. LESLIE MITCHELL.

**THE MIRROR OF EGYPT IN THE OLD TESTAMENT.** By LIEUT. COMMANDER VICTOR L. TRUMPER, R.N.R. *London and Edinburgh: Marshall, Morgan, and Scott. pp. 174, and 7 plates. 5s.*

Commander Trumper's book is intended for those 'who have the slightest desire for a fuller understanding of Scripture, and who may be interested in the land of Egypt, where the relics of antiquity throw such a vivid light on the allusions to Egypt in Holy Writ'. It is essentially, therefore, a book written by a layman for laymen and, within these limits, generously fulfils its purpose. The author has lived for more than twenty years in Egypt, and his observations of its life and customs, coupled with a keen interest in matters Biblical and archaeological, have helped him to appreciate the local colouring of the Old Testament stories. Those who wish to know how bakeries were organized in ancient days, how bricks were made, why Joseph was given royal linen, why silver is put before gold, and many other interesting sidelights on the narrative, will find here, together with a number of excellent photographs, much varied and unfamiliar information. It is a pity that the author, having decided to eschew dates, should have committed himself after several hesitant qualifications to the 'Ramses II' theory of the Oppression. Not only is this theory now widely challenged, but the view which makes Amenhotep II the Pharaoh of the Exodus is not without its own interest to the hunter of side-lights. We may, in fact, make Commander Trumper some return for an entertaining book if we refer him, *more ipsius*, to Breasted's History of Egypt (2nd ed., p. 327). He will find there a suggestion that Thothmes IV, who immediately followed Amenhotep II, *was not his father's firstborn and had at one time no expectation of succeeding to the throne!*

W. J. PHYTHIAN-ADAMS.

**YESTERDAY AND TODAY IN SINAI.** By MAJOR C. S. JARVIS. *Edinburgh: Blackwood, 1931. pp. 312 and 28 plates, 1 map. 15s.*

It is difficult to resist the temptation of quoting at large from the many entertaining and instructive things which the Governor of Sinai has to record about the 'Today' of that little-known peninsula; its sport, its smuggling, its locust-wars, its tribal laws and customs, and always, and not least, its round 25,000 of incorrigibly happy-go-lucky Beduin. Space, however, limits us to the 'Yesterday' which has made this region famous, and, more particularly, to the author's theory of the forty years' wandering of the Israelites. Very few scholars today maintain the traditional identification of Mt. Sinai with Gebel Mousa in the extreme south of the peninsula: and Major Jarvis brings

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further arguments against it out of the stores of a firsthand knowledge of the country. He himself places Mt. Sinai tentatively at Gebal Hellal 30 miles south of El Arish, and would refer the scene of the wanderings to the triangle of cultivable land between that town, Rafa, and El Kosseima. In harmony with this view, he advances the not altogether new theory that the 'Red Sea' of the Bible (the Hebrew 'Yam Suph' or 'Sea of reeds') was in reality the Bardawil Lake, a reedy lagoon on the Mediterranean coast not far east of Port Said. He suggests that it was along the narrow strip of land which divides this lake from the sea that Moses and the Israelites made their escape in safety, while the pursuing Egyptians were bogged by an inrush of sea, caused by the subsidence of the low sand-bar under a heavy east wind. Many critics, from Welhausen onwards, have urged that the real scene of the law-giving and the chief 'miraculous' events of the wanderings was to be sought in the vicinity of Kadesh Barnea (which Major Jarvis identifies with Ain El Gedeirat 3 miles east of Kosseima rather than with the more commonly accepted Ain El Kudeis): and some (*e.g.* Kittel) have wished to locate Sinai in the same neighbourhood. It is, therefore, of great interest to find this view put forward independently by one who, as an administrator, has had the opportunity of studying the problems so closely on its purely practical side. It is perhaps not possible to accept all his conclusions. His identification of the 'Yam Suph', for example, is open to the serious objection that whatever the original meaning of this name may have been, it was undoubtedly applied in the time of the monarchy to the Gulf of Akaba. (Curiously enough, the author notes a Mohammedan belief that 'the disaster occurred on the opposite side of Sinai in the Gulf of Akaba'. We should have been grateful here for references). Moreover, the careful distinction between the 'Way of the land of the Philistines' and the 'Way of the wilderness of the Yam Suph', the latter being chosen by Moses though the former was 'near', can scarcely be reconciled with the inconsiderable distance which separates the two routes necessitated by this theory. Of special interest is the author's suggested explanation of the Pillar of Cloud and Fire. He notes the occurrence in Sinai, when heavy weather is impending, of a remarkable cloud-formation, 'a huge column of cumulus, black in the centre with white edges. This column, which begins on the skyline and is most impressive, extends to the zenith, constantly emitting lightning, and at night is an intermittent blaze of fire'. Here again it may be asked whether such a cumulus 'coming in from the eastward' and presumably passing away over the Mediterranean, will satisfy the Biblical description of the Pillar which went before the Israelites and guided them on their long march. It may be unwise, however, to press every detail of an ancient tradition too closely; and we are grateful for a real effort to grapple with this enigma, which, apart from Gressmann's volcanic hypothesis, has not always been treated so respectfully. On the subject of the quails and the tamarisk-manna the Governor of Sinai may claim to write with authority, and we have little doubt that his conclusions on both these points will be endorsed by future research. His practical contribution to Biblical archaeology will be welcomed by students at home, but it is to be hoped that in succeeding editions he will not allow such forms as Kadish, Succouth, and Goschen to remain uncorrected.

W. J. PHYTHIAN-ADAMS.

VORGESCHICHTLICHES JAHRBUCH: BAND IV; BIBLIOGRAPHIE DES JAHRES, 1927. Berlin: Walter de Gruyter, 1930. *ph.* 420.

The delay in the appearance of this volume (it only reached *ANTIQUITY* in December 1930) is explained by the sudden death of the Editor, Prof. Max Ebert, wherein prehistory



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lost not only a great savant but an exceptionally gifted organizer. For this volume he had in most cases secured the services of eminent local authorities like Baron Loë (Belgium), Kostrzewski (Poland) and Björn (Norway) to compile the bibliographies of their respective countries.

The *Jahrbücher* are intended as supplements to the *Reallexikon*, keeping that work up to date by summarizing the results of investigations published since its appearance. The volume before us gives a very admirable and exhaustive survey of books and articles dealing with prehistory in most European countries (Austria, France and the Iberian Peninsula are omitted this year) and Hither Asia. The section on the British Isles is this time very complete and well-balanced. The heading 'Russia' affords a glimpse of the enormous mass of literature, some of it obviously of first-class importance to the world at large, which is being produced in the Soviet Union but is apparently absolutely unknown and unobtainable here; it is tantalizing that only the titles, happily in Cyrillic, with a German translation, but scarcely a hint of the content, should be given.

By reading through the relevant section of this volume the worker interested in any given area will be surprised at the gaps in his own knowledge but will be able to pick out precisely what he must read to fill them. More can hardly be expected; to make the bibliography strictly a supplement to the *Reallexikon* a very exhaustive subject-index, which must take years to prepare, would be needed. The existing index gives only the names of the authors whose works are cited and seems otiose. In addition to the bibliography, there is an article by Ungnad on the significance of the Royal Tombs at Ur, which is perhaps better on the philological than on the archaeological side, and a very handy account of the principal Swiss museums by Tschumi. It is altogether to be hoped that this indispensable bibliographical work may be continued.

V. G. CHILDE.

DIE ARTEN DER KÜNSTLICHEN SCHÄDELDEFORMATION. By J. IMBELLONI. *Anthropos*, vol. xxv, 1930. INTORNO AI CRANI 'INCREDIBILI' DEGLI INDIANI NATCHEZ: Atti del XXII Congresso Internaz. degli Americanisti, Rome, Sept. 1926. By J. IMBELLONI.

In his paper in *Anthropos* Prof. Imbelloni has enlarged the ideas expressed in the slight sketch read to the Congress of Americanists. This was the more necessary as the latter contained numerous serious misprints. The paper in *Anthropos* includes a series of very beautiful photographs of artificially deformed skulls. The author's method of classifying these is by taking tracings of the skull and recording the angles from various points, but it is somewhat unfortunate that at least one of his points cannot be actually seen from the aspect in which he makes his tracing. Except for three or four modern references and a bibliography of his own writings he has paid very little, if any, attention to the modern work on this most important and interesting subject.

L. H. DUDLEY BUXTON.

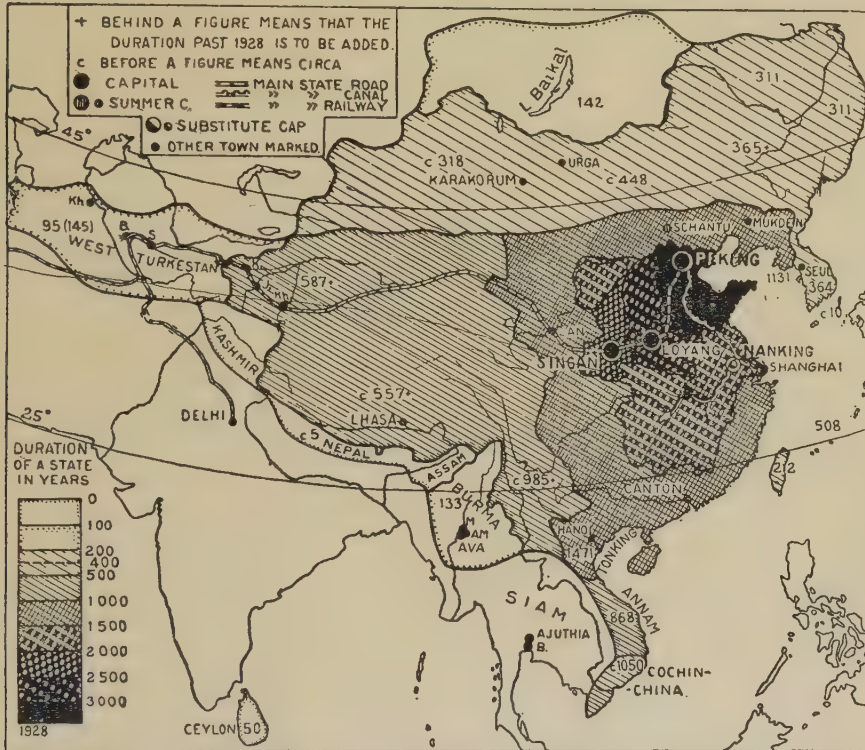
THE SUBTROPICAL BELT OF OLD WORLD EMPIRES. By STEN DE GEER. *Geografiska Annaler*, 1928, pp. 205-44. Stockholm: Centraltryckeriet, 1929.

This is a valuable and stimulating article. It belongs, as the author states, to the 'new political geography' which is succeeding the old, after a transitional welter of 'human geography'. He shows that the Old World Empires have originated and developed mainly within a subtropical belt lying between lat. 25° and 45° north. The

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frontiers of this belt may also be defined—on the north by the limit of vine-growing, and on the south by 'the isotherm of a fairly cool January ( $60^{\circ}$  F or  $15^{\circ}$  C) . . . The central line of the belt may be fixed as the northern limit of palms . . . It runs most significantly past Cordoba, Rome, between Constantinople and Memphis, through the group of Mesopotamian and Persian capitals, between Ghazni and Delhi, though near both of them, finally past Nanking and Kyoto' (p. 243).

The expansion and contraction of these Empires is illustrated by fourteen sketch-maps on a uniform scale of 1:40,000,000, and by a folding map of the whole area, full



of the most fascinating correlations but badly overcrowded and therefore almost illegible. The chief feature of these maps is the attempt to indicate the time-element of expansion and contraction by a system of shading, the regions being separated by 'isochronic' lines (the term is our own invention). Thus the problem of the fourth dimension, always so difficult for the historical geographer, is circumvented. When possible the main artery of each Empire and the capital cities are shown.

Perhaps the two most striking maps are those here reproduced (by permission), of the Chinese and Roman Empires. The nuclear area where political power originated and survived longest—the political centre of the Empire—is shaded darkest; it is these nuclear areas that are most important of course.

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In so all-embracing an essay minor discrepancies are inevitable. It seems unnecessary to particularize the Assyrian Empire as 'founded on force'. It was, of course, but then so is every other Empire. The 'more or less regular raiding and sacking' of the border provinces differed from other imperial exploitations only in being more open and clumsy and therefore less effective in the long run.

It is interesting to find that Egypt is 'closely bound to the other Mediterranean and East Mediterranean countries' rather than to the 'West Asiatic centre of Empires' (p. 231). This conclusion, derived quite independently from geographical study,



The Roman Empire. Scale 1 : 40,000,000. (After De Geer)

agrees entirely with Sir Flinders Petrie's conclusion from the historical view-point.\* Indeed Mr De Geer almost restates it when he says that 'the area of East Mediterranean States and civilization has for several centuries been designated by the regional name the Levant. It has long been considered a national unit'. A treatment of the wave theory of civilization on geographical lines analogous to these followed here by Mr De Geer would be very illuminating, and we hope that some day he may undertake it. Only by work on such lines can broad generalizations be established; and it is only by means of broad generalizations that history can be rescued from pedants.

\* 'The phase of the wave of civilization was identical in Egypt and Europe to within a century, where it can be observed in three periods . . . The Mediterranean and Egypt, as a whole, form therefore a single group in the history of civilization'. (*Revolutions of Civilization*, p. 83).



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THE TOPOGRAPHY AND HISTORY OF BETH-SHAN. By ALAN ROWE.  
*Philadelphia: University Press, 1930.*

THE FOUR CANAANITE TEMPLES OF BETH-SHAN: part II, The Pottery.  
By G. M. FITZGERALD. *ib.*

BETH-SHAN EXCAVATIONS, 1921-1923: the Arab and Byzantine Levels. By  
G. M. FITZGERALD. *Philadelphia: University Press, 1931.*

THE THIRD WALL OF JERUSALEM. By E. L. SUKENIK and L. A. MAYER.  
*Jerusalem: University Press. London: Oxford University Press, 1930. 15s.*

The publications before us are most welcome additions to the literature of research in Palestine, though in these hard times we cannot help asking whether it was necessary to publish them on so magnificent a scale. The Beth-Shan volumes are only the first fragmentary instalments of a final publication of one of the greatest excavations in the country, and we tremble to think how many other volumes will be required if this standard is maintained. 'The Third Wall of Jerusalem', on the other hand, is an account of what was in itself only a small undertaking which the authors were debarred by want of funds from completing as they wished. It is a much less sumptuous book than the Beth-Shan books, but even in this case we wonder how the money expended on the work compares with that expended on the publication.

The excavation of the *tell* at Beisan was started by Dr Fisher in 1921 on behalf of the Museum of the University of Pennsylvania; in 1925 Mr Alan Rowe, the author of the first volume, took over the work as Field Director, and he was succeeded in turn last year by Mr G. M. FitzGerald, who is the author of the second volume. In spite of these changes and though work has been suspended for a year or two more than once, there has been no serious breach in the continuity of the supervision, for Mr Rowe and Mr FitzGerald were members of Dr Fisher's staff and Mr FitzGerald remained with Mr Rowe after Dr Fisher had left for Megiddo.

At present Beisan is an insignificant little town: the railway from Haifa to Damascus passes through it, but the trade with Damascus is no longer of much consequence and the important through lines of communication in Palestine are those which run north and south. In old days it was not so, and from the second millennium B.C. down to the Middle Ages Beisan, which commands the Jordan end of the easiest east-west route through Palestine, was a great centre by virtue of its position and the fertility of the surrounding country.

Mr Rowe's volume includes a full account of the different levels on the *tell* and a more summary account of the temple found there with the very important hieroglyphic inscriptions belonging to them, and a most useful collection of the literary references to the place. It is illustrated with more than fifty plates which are the work of the Boston Heliotype Co.

The greater part of the Roman and later town lies on the hills round the *tell* but the *tell* itself, which was the early centre, was occupied down to the Arab period and the upper strata are like the upper strata in many other Palestinian sites. The use of cut stone introduced on a large scale first perhaps in Hellenistic times provided a material which could be re-employed again and again, with the result that clean sweeps were made of whole series of buildings, and the deep foundations which were laid here as elsewhere in the late Roman and Byzantine periods worked further havoc with these

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strata. It is only when one gets back to the earlier period when mud bricks were in use and the comfortably familiar processes of what has been called *tellification* began to operate, that we reach a series of well-marked levels with reasonably substantial strata between. According to the schematic diagram on plate I there are five levels between 1501 and 1225 B.C. and in less than three hundred years the *tell* rose some five metres in height, whereas in the very much longer period since 301 B.C. it has only risen half this amount at the highest point. In the earlier strata the most important finds are the great series of superimposed Canaanite temples which Mr Rowe has reconstructed with extraordinary ingenuity, a fine Assyro-Babylonian relief in two registers each with a lion and a dog, the stele of Mekal, those of Seti I, inscribed lintels of the time of Ramses II, a stele of the same kind and other inscribed fragments, the whole forming a wonderful series of monuments especially in a country so poor in inscribed material as Palestine. Illustrations of these finds are published in this volume but Mr Rowe promises us a more detailed account in the final publication of the Four Canaanite Temples which is reserved for volume II of this series.

Mr FitzGerald's volume forms the second part of this volume II and it is a masterly account, with excellent line illustrations, of all the pottery found on the *tell* except the cult objects. The account in fact is so good that we cannot refrain from expressing our regret that the pottery from the graves has not been published in the same volume.

The third volume of *Beth-Shan Excavations* deals with the Arab and Byzantine antiquities which were found on the summit during the seasons 1921-1923. This book, like volume II B, is by Mr FitzGerald and the work could not have been placed in more competent hands, though Mr FitzGerald was only at Beisan for a short portion of the period in question. Antiquities of this date are so often treated by archaeologists in a cavalier fashion that this careful and lucid account of both buildings and objects is doubly welcome.

It will not surprise those who are familiar with Palestinian sites to read that, owing to continuous occupation, very little of the buildings remained and it was impossible to ascertain the original ground plan of a single Byzantine house. Two buildings of some interest were however found, a gate flanked with towers at the northwest corner of the *tell* and a remarkable circular construction on a higher level. To the latter, which Mr FitzGerald identifies as a church, two chapters are devoted. The building in question consisted of a circular colonnade or *stoa* with an entrance on the west side and an apsidal extension on the east. Hardly anything was left above ground level and the reconstruction of the original structure is a matter of some difficulty. The editor argues with reason that the central area was never roofed: the width of the space to be spanned—more than 25 metres, the slightness of the surviving columns and the wall on which they rested, and the existence of a drainage gutter in the southwest section of the colonnade, are proofs in point. This being the case, however, are we justified in calling the whole construction a church? Admitting that the apsidal extension at the east may be a chapel, we should prefer to describe the colonnade in front of it as a circular atrium: it is none the less interesting on that account. It is lamentable that none of the fittings of this apsidal extension were found: if it was a chapel arranged like those at Jerash, the altar would have stood on the chord of the apse and there would not have been more than one step between it and the part further west, which would have been a *solea*. The colonnade was destroyed before A.D. 806 and not a single column base was found in position. Mr Fitzgerald argues that there were only six columns on each side,

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but the intercolumniation which this number gives (4.06 m.) is very wide even for a wooden entablature, and we are inclined to assume double the number of columns. The geometrical principles on which the plan was set out are convincingly explained in the second chapter on this building, which should be studied by all who are interested in the development of East Christian architecture.

Of the objects found the most interesting are a fine series of bronze polycandela, a bronze lamp standard, a bronze censer, a stone mould for baking Eucharistic bread, and the incised and painted Arab ware shown on plates xxvi and xxvii. The inscriptions in Greek, Latin, Arabic and Hebrew, are adequately published, and the volume forms a notable addition to our knowledge of the period.

The title of the fourth volume recalls Josephus' account of the siege of Jerusalem : Josephus says that there were three walls on the north side of the city, that the third and most northerly of these was begun by Herod Agrippa, abandoned by him from fear of displeasing Claudius, and finally completed in great haste after the outbreak of the 'first revolt'. The line of this wall has been the subject of an old controversy : some identified it with the existing north city wall, others with the remains further north which were described by old travellers such as Pococke and Robinson but have since disappeared.

This controversy was revived by a chance discovery made in 1925 close to the site of the present Government Museum, and the excavations described in this volume were undertaken in the hope of settling the question finally. A series of modest trenches brought to light a line of walling extending over a distance of more than 500 metres : in one place the wall was traced for a continuous length of 81 metres and its general character was clearly established. It was from 4 to 4½ metres wide and defended by towers ; there was a gate just west of the American school ; the masonry was curiously irregular : a bedding of small stones lay between the rock and the first course which was the only one of which much remained ; in this course some stones were of great size and admirably dressed, one of them measuring over five metres long, others were of an inferior character. An attempt was also made near Herod's Gate to find where this wall cut the present north wall but the excavators were unfortunately compelled to abandon this before any conclusive result was reached.

The work actually accomplished is very clearly described, the relevant literature is quoted and an excellent series of photographs and plans are provided : the reader therefore has all the material available to appreciate the bearing of the new finds. To the present writer it seems that Dr Sukenik and Dr Mayer have made out a strong *prima facie* case for their hypothesis ; and they have shown that this wall occupied an excellent strategic line well calculated to defend the northern suburb of the Herodian city. The masonry, it is true, is too irregular in character to be identified as the work of Agrippa but, as the writers argue, it is uncertain how much Agrippa really completed—in parts the wall may have been almost finished, in parts only the foundations or not even these may have been laid—and in any case it is clear from what happened in the times of Florus and Cestius that as a whole this wall presented no serious obstacle until after the outbreak of the 'first revolt'. The character of the walling actually found is consistent with our writers' suggestion that this section at least was hastily assembled to meet a desperate emergency, like the Third Wall of Josephus. It is much to be hoped that Dr Sukenik and Dr Mayer may be able some day to close the question finally by completing their investigations near the existing north wall.

J. W. CROWFOOT.



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### HISTORY OF PALESTINE AND SYRIA TO THE MACEDONIAN CONQUEST.

By A. T. OLMSTEAD, *Professor of Oriental History, University of Chicago.* Charles Scribner's Sons, 1931. pp. xxxiii, 664, with 188 illustrations, 18 plans, 1 map. 30s.

The historian of ancient Palestine and Syria is faced, at the outset, by one almost insuperable difficulty. His province, thus geographically divided, falls into two sections widely divergent both as to extent and as to international significance. Syria, if with Prof. Olmstead we prolong it as far to the north as Marash, not only contains an area more than three times as large as that of Palestine but completely outvies its smaller neighbour in the size, the number, and the importance of its ancient sites. There is, in fact, no comparison between the two countries when we estimate the parts played by them in the secular history of the Near East. But when the historian turns to the documents which must provide him with his material, he has to deal with a situation in which these rôles are completely reversed. Of the great civilizations of Carchemish and Kadesh-on-the-Orontes, of the brilliant coastlife of Byblos, Tyre, Simyra, and a dozen other seaports, of the rise and fall of Amorite, Hittite, and Aramaean dynasties he can win no more than a series of brief and intermittent glimpses, whether from the disinterment of their ruins or from the terse records of their successive conquerors. It is only when he is considering the vicissitudes of Israel and Judah that he finds not merely a sufficiency but even an embarrassing wealth of documentary evidence. And here further difficulties confront him. He has to face, first, the task of secularizing what is for many of his readers a Sacred Book and to determine how far its predominant interest in religious problems is to be treated as relevant and essential. He has to decide, secondly, how best to exercise a wise economy in re-telling a story at once familiar and profuse in detail. Finally, on a general survey of the material at his disposal, he must make choice between the possible alternatives, whether to accept the history of Israel as the central interest of his work, to set it boldly in the fore-front of the larger canvas, or to allow it to take its place—and that of necessity a small one—in the crowded panorama of contending empires. The mere statement of these multifarious problems is enough to indicate the magnitude of the historian's task, and it is no carping belittlement of Prof. Olmstead's achievement to question whether he has satisfactorily solved them. The book he has given us seems curiously unbalanced and indecisive. If his purpose was to present an objective picture of the ancient civilizations of Syria (using that term now in its widest sense), he must incur the charge of having greatly overloaded his book with Old Testament matter. Details of such minuteness as the wise-woman's cry at Abel (to take merely one instance at random) are surely out of place in the survey of so wide a field, and a very large amount of the space devoted to prophetic utterances could, on this view, have been equally well spared. If, on the other hand, Prof. Olmstead's aim was to emphasize the ethical and religious superiority of Israel in contrast to the paucity of its resources and the greater brilliance of the nations surrounding it, it can only be said that he does his best to conceal this. We receive, rather, the impression of a small people struggling like its neighbours for greatness and only differing from them in being continually thwarted by its own religious leaders; and we feel inclined to ask whether these domestic conflicts deserve to bulk so largely in a history of the ancient Near East. The author does not, in fact, seem to justify on either view his copious reproduction of the Biblical narrative. One point of historical criticism must be mentioned. In dealing with the period of the Judges, Prof. Olmstead conceives himself at liberty to 'adjust' the various episodes without reference to their present

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position in the text. Curiously enough Prof. Garstang in his recently published work on this same period has demonstrated with great plausibility the essential integrity of the narrative and its remarkable corroboration by the contemporary Egyptian records! So complete a contradiction in theory as this will make the reader wary of accepting too confidently the critical conclusions assumed rather than advanced throughout the present work. For the student, indeed, its value will very largely reside in the mass of new evidence, archaeological and epigraphic, with which recent excavations have provided us. On this score alone the book will be for long indispensable, for Prof. Olmstead has spared no pains in collecting and presenting the latest information from these sources. His illustrations are both numerous and excellent, though we should have been grateful for more (and better) maps, especially in those chapters which concern the geography and history of Syria. A few misprints have been noted: p. 119 'course' for 'coarse'; p. 336 (last line) 'beings' for 'begins'; p. 434 'Quirl' for 'Quril'. The description of the vases in fig. 121 is misleading, for only three of these can correctly be termed 'Philistine pottery'.

W. J. PHYTHIAN-ADAMS.

THE QUARTERLY OF THE DEPARTMENT OF ANTIQUITIES IN PALESTINE. Vol. 1, nos. 1 and 2. *Jerusalem: Published for the Government of Palestine by Humphrey Milford, Oxford University Press, 1931. 5s each, yearly 18s 6d post free.*

The appearance of this new periodical will be greeted with satisfaction by archaeologists, though the fact that it is due to the munificence of Mr J. D. Rockefeller, Jr., will be viewed by those of them who are English with mixed feelings. 'Its main purpose', to quote the introduction by the Director of Antiquities, 'is to publish (a) any discoveries resulting either from excavations carried out by the Department, or from other methods of research, or that come to light in a more accidental manner in the course of the Department's ordinary administrative work; (b) notes upon such antiquities in the Department's Museum, or elsewhere in Palestine, as have not already been published; (c) texts and translations of texts describing historic monuments and sites; (d) general news of archaeological work in Palestine'. The present numbers, accordingly, contain notes on a cemetery at Karm Al-Shaikh, Jerusalem, on recently discovered hoards of Phoenician and Byzantine coins, on a rock-cut tomb at Nazareth, and on a selection of coins in the Palestine Museum. To these are added articles on the medieval Ajlûn and the medieval Arabic description of the Haram of Jerusalem, and a collection of Arabic inscriptions 'unpublished and inadequately published' of which the Department possesses squeezes and photographs. Pre-Roman archaeology is represented by a 'Concise Bibliography of Excavations in Palestine', which, when completed, will be extremely useful to scholars. It will be seen from the above that so far the Arabist and the Numismatist divide the feast pretty equally between them, for the few crumbs of Roman and Byzantine glass and pottery offered will not go far towards assuaging the appetite of Stone-, Bronze-, and Iron-Age students. It is to be hoped that in future numbers the Department will redress these proportions so as at least to preserve a balance of interests. Meanwhile we extend a hearty welcome to the new venture, which is beautifully illustrated and produced in a style worthy of its future importance. The incorrect plate-references which will be noted in no. 1 pp. 3-5 are corrected in an erratum inserted in the second number.

W. J. PHYTHIAN-ADAMS.



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### Roman Gaul

*We have received from Professor Grenier a rejoinder to the review published in ANTIQUITY for September 1931 under the title of 'Roman Gaul'. With his assent this has been translated by the Editor, and submitted to Mr Ian A. Richmond, the writer of the review, whose reply to Professor Grenier is appended so that both may be read together.*

The article 'Roman Gaul: a Review' (ANTIQUITY v, Sept. 1931, 344-350) contains numerous mistakes. It was not at Vienne that there were found sculptures parallel to those of St. Remy, but on the triumphal arch of Orange. There is no Roman crenellation at Saintes. Neither have I seen any at Fréjus near the Porte des Gaules, and I have never penetrated into the Villa Léonine whose owner does not like archaeologists. But I have had the facts verified; and I am told that there is nothing. Doubtless that is why there has been no reference to them. The author of the review is equally unfortunate in his denial of the existence of the *castellum* of Senon, near Verdun. There can be no doubt whatever about it. I have seen it; Liénard and Chenet have excavated there, and also Drexel, during the War, in the course of his duties as Gefreite of the Landwehr.

It would be easy to quote other instances of statements or denials equally rash. The criticisms are no surer.

The *titulus* is said, on p. 346, to be a 'distinctive test for Roman work'. By *titulus* is meant either a ditch (*e.g.*, as at Heddernheim) or a series of small parallel ditches (*e.g.*, as at Xanten), placed in front of the gate of the camps. But such works are often absent. They are, in fact, an additional defence, disposed according to circumstances; in cases of siege they were as awkward for the defence as for the attack, for they prevented a sortie being made. The reviewer says: 'The omission of the fine long one at Urmitz is not excusable today'. That certainly is not the opinion of M. Lehner of Bonn, who has proved that this great ditch of Urmitz belongs to the neolithic earthwork (*enceinte*), in an angle of which the Roman camps are situated. It would have been wiser, after such a blunder, not to speak of Glazelianism.

It is proper to state that similar works, in Arabia and in the west, are not necessarily contemporary. There is no object in comparing works which differ both in date and character, *e.g.*, Eysses and Cirencester; Nérès and Inchtuthill or Haltern. Of Eysses nothing is known except that there have been found there some inscriptions of auxiliaries, probably of the beginning of the 1st century. Cirencester is a town where a good deal of digging has been done, and whose period of prosperity dates from the 2nd century. Nérès was a watering-place; Inchtuthill is a camp where, as often elsewhere, a military bath-house has been found. There are traces at Nérès of two camps, one of which looks like a prehistoric promontory fort, whilst the other (much smaller) remains undated. Neither of them has the slightest resemblance to the two Augustan camps of Haltern.

To understand the plan of the 1st century Gallo-Roman towns and the course of their enclosing walls (*enceinte*) one must bear in mind the distinction, classic since Mommsen, between Roman and Latin *coloniae*. The former were for the most part colonies of veterans; they preserve their wholly military character (Tac., Ann. xiv, 27, 4). It is legitimate to speak of *castra* in connexion with them. But it is quite otherwise with Latin colonies, whose population consisted mainly of natives. Vienne, which no



## REVIEWS

one has ever thought of comparing to a camp—Nîmes—resembles Autun which had not the name of colony; and all betray by their shape and plan certain points of resemblance to the one Celtic *oppida*. The towns of Roman Britain present moreover the same contrast.

It is true that we have no good archaeological plan of Nîmes later than the 18th century. The *Forma Orbis Romani*, directed by A. Blanchet, whose first map has just appeared,\* will shortly provide one. No doubt we would gladly have details of that iron grill of the Porte des Eaux, found at Nîmes in the 16th century; but unfortunately it has since disappeared. But let us not revive defunct hypotheses in a still more improbable form. It is long since the attempt to make the Tourmagne into a signal-tower was given up. Let us not now make of it 'a great Pharos-like tower', to light up the waste land round Nîmes presumably. It is known that, inside the tower, the lower stages have completely disappeared. It is perhaps rash to conclude from their personal arrangement that the monument can only date from the time of Augustus, and to reject the results of the patient observations of Espérandieu and his predecessors; but this is what the reviewer does when he states that, in spite of the absence of any connecting link in the masonry, the Tower was designed to form part of the rampart.

So far as fortifications of the 3rd and 4th century are concerned, there are close resemblances between Gaul and Britain, in spite of their divergent history. But there is an element essential in making comparisons between the fortresses of the two provinces—the dimensions of those fortresses. One must not compare a fortified town (*castrum*) with a *castellum*, a mere roadside fort, posting station and *horreum*. There is no 'Rhine-land' type of fortification. On the banks of the Rhine we find the same plans and the same architecture as inland; and no one has ever questioned their military origin.

'Why Tournus should be compared with Alzei rather than with Andernach passes the comprehension' of the reviewer. A little closer attention would have enabled him to understand the point of this comparison—in which moreover there is nothing essential—and many other more important matters.

One of my friends, an architect, once declared, in front of some Roman ruins—'Ancient architecture is quickly seen'. It seems that confronted with Roman Gaul the reviewer is like the architect.

### Reply by Mr IAN A. RICHMOND:—

I have read Professor Grenier's reply to my review with great interest, and grieve that he should have misunderstood some of the points raised by me with greater interest in the subject than he has been willing to concede.

The Vienne sculptures are, of course, nothing like those of St. Remy in subject, but that was not the point at issue. I was drawing attention to Gallic feeling which both sets of sculpture exhibit and of which the variety is illustrated by the very diversity of subject. Nor did I deny the existence of the structure at Senon. I doubted, and still do, whether it is essentially a fortress. Again, the Urmitz *titulus*, to which I called attention, appears on the official plan in Bonn Museum just outside the west gate of the smaller Roman camp, where it is omitted on Professor Grenier's plan; and anyhow, while he rightly observes that permanent works often lack *tituli*, the context in question

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\* Doubtless M. Grenier's statement is correct, but there is no knowledge of this map in Britain, nor has a copy been received by the translator in any of his various capacities.—O.G.S.C.



## ANTIQUITY

dealt with *castra* and my contention was that these, axiomatically, should have either *claviculae* or *tituli*.

The parallels with which fault is found were mentioned to arouse the interest of fellow-readers, as they aroused mine, always with the assumption that Professor Grenier would know them. But it must now be mentioned that Eysses and Cirencester were both important road-junctions, and that they have both yielded auxiliaries' tombstones belonging to their earliest days, thus raising the same problem of interpretation of these relics. Again, the site of Nérès, with its promontory fort, its two Roman fortifications and its unattached barracks, is strikingly like Inchtuthill, where there are not only *two* bath-houses, but (a) a promontory fort (b) a large camp (c) a small camp and (d) barracks unattached to either (b) or (c). Surely, too, this is reminiscent of the tangle of *Uferkastellen* at Haltern.

The Tacitean citation refers to obsolete conditions which were once (*olim*) typical, no doubt under the Republic and in Cicero's day. But where is there in all Gaul a castrametated colony of this type? And how can the contrast between *coloniae romanae* and *latinae* be illustrated in Britain, where there is not one town of the latter class?

Passing to Nîmes, my observations upon the structure of the Tourmagne are based entirely upon the *visible* remains, which anyone can visit today and which cry out for adequate publication. I did not assign this monument exclusively to Augustus, but ventured to date it not earlier than his day. Finally, my equation with the Pharos was one of form and not of purpose, and, while its value may be disputed, its truth can hardly be denied.

In dealing with fortifications, I may refer to Blanchet (p. 260) for the parapet near the Porte des Gaules at Fréjus. It is still there, and if ever my critic penetrates the grounds of Villa Léoncine, he will not miss the other example, as his informant appears to have done. In the discrimination of fortresses Anthes's plans are my witness that the round-towered fortresses are typical in the 4th century Rhineland. Meanwhile, the most careful attention does not yet persuade me that regular Alzei is the pattern for irregular Tournus, whether in purpose or type; for the comparison offends against the canons now advanced by its advocate.

My concluding action, however, must be to thank my critic for his interesting discussion. The mischance that some of my points were insufficiently clear has been the happy opportunity of providing at least one reader with a delightful specimen of courtesy and erudition, *d'autrefois*.

IAN A. RICHMOND.